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CONTENTS

An experiment on the effect of specially designed safety posters. By S. LANER and R. G. SELL.

Speed and power factors of intelligence: further evidence. By O. R. POREBSKI.

A reply to Dr. Porebski. By D. F. VINCENT.

Dr. Remitz on white-collar satisfaction. By JOHN D. HANDYSIDE.

Food assessment and food acceptance as a psychological theme. By R. HARPER.

Bias in the measurement of food preferences by triangular tests. By R. A. M. GREGSON.

The development and use of a taste panel technique. By A. S. C. EHRENBERG and J. M. CHERVAN.

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T. H. PEAR. The accuracy of 'reproduced' conversation.

NORMAN F. DIXON. Apparent changes in the visual threshold: central or peripheral?

SATORU AIBA. The effects of stimulant and depressant drugs on the Bidwell phenomenon.

R. LYNN. Extraversion, reminiscence and satiation effects.

D. W. FORREST. Association between muscular tension and work output.

R. G. CHATTERJEA. The effect of distraction on time gap in the estimation of short duration.

HARRY M. B. HURWITZ. The effect of illumination conditions on the effectiveness of light-onset as a reinforcer: a test of the Bevan-Adamson reinforcement theory.

D. F. BROOK and A. W. HEIM. A preliminary note on the Brook reaction test.

CHRISTOPHER ALEXANDER. A result in visual aesthetics.

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THE QUARTERLY JOURNAL OF
EXPERIMENTAL PSYCHOLOGY

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CHOICE REACTION TIMES FOR SKILLED RESPONSES*

BY

G. H. MOWBRAY

From the Johns Hopkins University Applied Physics Laboratory, Silver Spring, Maryland

Two kinds of choice reaction time experiments are reported, both of which make use of a highly overlearned sensori-motor response. When a response is required for each stimulus presented, no increase in reaction time occurs as a function of the number of alternative stimuli available. It is proposed that the increase in choice reaction times commonly thought to accompany an increase in the number of alternative choices provided reflects the unpractised state of the responder. When a response is required for only one out of n possible stimuli, a slight but consistent increase in reaction time takes place with an increase in the number of alternatives. An analogy is drawn between the second experiment and a vigilance task and an expectancy hypothesis is invoked to explain the results.

Introduction

It has long been accepted as fact that choice or disjunctive reaction times increase with the number of alternative responses required. The generality of this dictum apparently has been accepted without question since the early experiments of Merkel (Merkel, 1885; Woodworth, 1938). Further impetus to its acceptance has been provided in recent years by those interested in applying communication theory techniques to the analysis of human behaviour (Hick, 1952; Crossman, 1953; Hyman, 1953). However, as data have accumulated from diverse investigators using different techniques of stimulus presentation and response, it has become increasingly clear that qualifications to the basic generalization are needed. As an example, Pierce and Karlin (1957) have shown that reading rates do not increase when the size of the vocabulary used is increased, and most recently Leonard (1959), in a cleverly conceived experiment, has produced data on tactual choice reactions that show no increase in choice reaction time as the number of alternatives is increased from two to eight. Previous to Leonard's finding, Mowbray and Rhoades (1959) had demonstrated that the difference in reaction times between a two-choice and a four-choice finger response could be reduced to zero by intensive practice. Finally, Saslow (personal communication), using recorded speech syllables as stimuli and repetition of the stimulus as the response, has obtained reaction times that do not change as a function of the number of alternative responses required.

One question that arises from these results might be stated in the following way. Is the reported finding that choice reaction times increase with increasing numbers of alternatives a reflection of the unpractised state of the responders when a complex neuro-muscular response is required? It is certainly true that those

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experiments that show the greatest effect due to increased numbers of alternatives also utilized the most unnatural or unfamiliar stimulus-response combinations (cf. Merkel, 1885; Hick, 1952; Crossman, 1953; Hyman, 1953). On the other hand, in those cases where small increases or no increases at all were encountered, the subjects were either highly practised or else familiar stimulus-response combinations were employed (cf. Crossman, 1955; Pierce and Karlin, 1957; Mowbray and Rhoades, 1959; Leonard, 1959).

A shortcoming common to nearly all of the researches carried out to date concerns the small number of subjects tested. Understandably, it is tedious and sometimes prohibitively uneconomical to use large numbers of observers where a vast amount of practice is required or where many experimental conditions are used. However, it is dangerous to generalize from a limited sample.

Some of the above considerations were uppermost in mind when the experiments to be described were designed. To avoid the tedium of long practice sessions, a task was sought that required a fairly complex neuro-muscular response to a stimulus that was compatible with the response. The task finally chosen as the one most nearly meeting all requirements was the vocal identification of visually presented Arabic numerals. Undeniably that is a perceptual-motor skill that gets thoroughly over-learned by most literate people. By using this type of task it was hoped to achieve two things. First, by taking advantage of an already highly over-learned skill, it was reasoned that a more definitive test could be made of the hypothesis that choice reaction time is an increasing function of the number of alternative responses required. Second, the savings in time afforded by the use of an over-practised perceptual-motor response would permit the use of a larger number of observers than has been customary to date.

It should be noted at this point that Alluisi, Muller and Fitts (1957) report data on the vocal naming of visual numerals in a forced-paced serial task. Interestingly enough they found no differences between relative rates of information transmission with changes in the number of possible alternative stimuli over a range of 1 to 3 bits/stimulus. They were, however, not concerned with reaction times to individual stimuli nor did they even record such data.

Experiment I

Apparatus and procedure

The apparatus consisted of a teletype, punched-tape transmitter and a bank of coding relays that supplied the anode voltage to a Burroughs read-out tube. These tubes, called Nixies, contain tiny wire filaments in the shape of the numerals 0–9, and when the circuit through any one of these filaments is completed, the appropriate number glows brightly enough to be seen easily in a normally lighted room. They are a gas-filled, cold cathode tube whose light-up time is virtually instantaneous. They extinguish equally rapidly. The numeral they produce is 0·6 in. in height with a stroke width-to-height ratio of one to eight.

Standard electronic timing circuits generated a pulse that triggered the tape transmitter every 2·5 sec. so that a new stimulus appeared that often. The transmitter triggering pulse also operated the clutch of a Standard Timer Model S-1. The clutch of the clock timer was de-energized and the clock stopped when the subject responded by naming the lighted numeral. This was accomplished by an electronic voice key operated from a throat microphone worn by the subject. All voice reaction times were measured to the nearest one-hundredth of a second. Delays caused by relay inertia were measured and found to be 65 millisec. on the average. Consequently, all reaction times reported were corrected by that amount.

For this experiment five different degrees of choice were provided—2, 4, 6, 8, or 10 choices—and any one subject was assigned randomly, with certain restrictions, to only one of the five possible conditions. Thus a subject who was assigned to the two-choice

condition saw, and of course, responded to one of two possible numerals. Reaction was the simple vocal naming of the numeral that appeared on the read-out tube. A total of 150 subjects participated in the experiment with the only restriction on the randomness of assignment to conditions being that an equal number of males and females be represented in each choice level. Therefore, there were fifteen males and fifteen females in each of the five choice conditions under test. The subjects, with the exception of a few laboratory technicians, clerical assistants and college faculty members, were college students. Their median age was 19·5 years with a range of 17–48 years. Most had never previously served in any type of psychological experiment. Table I shows a distribution of the ages of all of the subjects for each of the five experimental conditions.

TABLE I

DISTRIBUTION OF THE AGES OF THE 150 SUBJECTS FROM EXPERIMENT I FOR ALL EXPERIMENTAL CONDITIONS

Experimental conditions	*Age* 17–20	21–24	25–28	29–32	33–36	37–40	41–44	45–48
2 choices ..	19	7	2	1	1	—	—	—
4 choices ..	14	11	2	1	1	—	—	1
6 choices ..	17	9	2	—	—	2	—	—
8 choices ..	20	7	1	2	—	—	—	—
10 choices ..	18	9	3	—	—	—	—	—

The subjects were seated facing the read-out tube and at a distance of 7–8 ft. with the tube at approximately eye level. Before each experimental trial commenced, the subject was told how many numbers he would see and what those numbers were. Stress was placed on the fact that those were the only numbers he would see, and if there appeared any doubt or hesitation it was made certain that the subject understood the task and, more important, that he knew which numbers he was going to be shown. Each experimental run was composed of 60 repetitions of each of the numerals included in a particular condition, thus insuring equal probability of appearance for all numerals. Therefore, subjects assigned to the two-choice condition provided 120 responses whereas those assigned to the ten-choice condition provided 600. The order in which the numerals appeared was in all cases random.

In analyzing the results only the reaction times to the appearance of the numeral 8 were used since this was the only number common to all conditions. The 8 was chosen originally because, beginning with a vowel sound as it does, it provided more immediate acoustic power for operation of the throat microphone than most of the other numerals. Finally, only the responses to the last 50 key stimuli were used—not counting errors. This procedure allowed for a brief but sufficient familiarization and warm-up period.

TABLE II

MEAN CHOICE REACTION TIMES AND STANDARD DEVIATIONS FOR 150 SUBJECTS AS A FUNCTION OF THE NUMBER OF CHOICES INVOLVED. TIMES ARE IN SECONDS

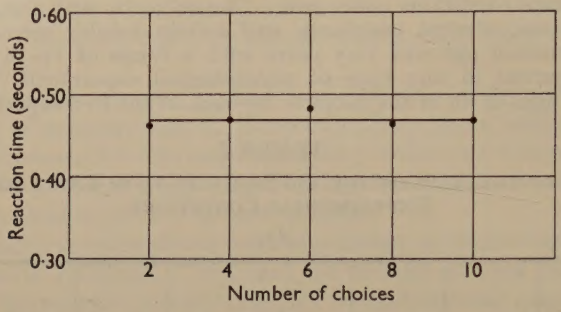
Number of choices	2	4	6	8	10
Mean reaction time ..	0·461	0·469	0·481	0·463	0·464
Standard deviation ..	0·075	0·060	0·045	0·078	0·065

Results

The mean choice reaction times and standard deviations obtained for the various degrees of choice are given in Table II. The same data are plotted in Figure 1 where the least squares fit to the data points is shown. As can be seen, the slope

of the least squares fit line is zero. That demonstrates, in effect, that choice reaction time is a constant for an over-learned response where as many as ten choices are involved.

FIGURE 1



Mean choice reaction time as a function of the number of choices available to the responder. Each data point is the mean of 50 reaction times from 30 different subjects.

The reaction times of all subjects were submitted to further analysis to determine whether the vocal naming response utilized in this experiment was indeed an over-learned one. To accomplish this, the mean times for the first 25 scored reactions were compared with the mean times for the last 25 reactions for all subjects and all degrees of choice. The mean differences so obtained were then tested for significance using the method recommended for correlated means (McNemar, 1949). None of these differences in any way approached significance. Thus it can be concluded that the response being measured was over-learned to the point that no further improvement took place throughout the course of the experiment.

Finally, a comparison was made between the reaction times of the male and female subjects. It was found that the females on the average reacted more slowly than the males by a nearly constant amount. The average difference of approximately 50 millisec. was significant at beyond the 0·001 level of confidence.

Experiment II

The experiment described above has dealt with but one of the two classical types of choice or disjunctive reaction. The other type requires the subject to react to only one stimulus of two or more that might be presented to him. Thus, in this latter case, the subject knows what the reaction will be; however, he does not know when it will be required. A second experiment of this latter sort was designed and performed. The apparatus and the experimental material were the same as in Experiment I. The subjects were 74 of the 150 who had served originally, but they were assigned to a different experimental condition than the one they had served in previously. Originally, it had been planned to use 75 subjects but due to a scheduling error only 74 were run, so that there were finally 15 subjects in each of 4 of the experimental conditions and 14 subjects in the fifth condition. There were 37 males and 37 females divided nearly equally between the 5 experimental conditions.

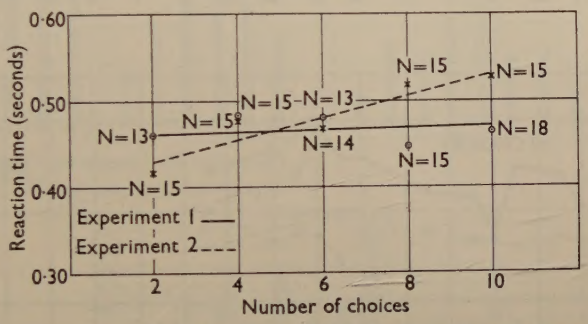
The procedure was identical with that described for Experiment I with one important exception. In this second experiment the subjects were required to respond only to the appearance of the figure 8. Thus, although they saw all of the numbers that were used for a given choice condition they could ignore, in so far as responding was concerned, everything except the appearance of the 8. This stipulation provided for the subject one complication that was not present in the first experiment. It caused an uncertainty about the time that a response would be required. In Experiment I, a response was required every 2·5 sec., but in this latter case responses were randomly distributed with a mean response interval that increased as a function of the number of choices.

Results

The mean reaction times for all subjects who participated in Experiment II are plotted in Figure 2 as a function of the number of choices involved. They are the points connected by a dotted line. Also plotted, as a solid line, are the mean reaction times of the same subjects from Experiment I. The *N*'s for the data from Experiment I differ somewhat from point to point because the subjects for Experiment II were selected at random from among those who had participated in Experiment I. They were not selected on the basis of what condition they had seen previously. It was purely accidental that the *N*'s turned out to be so nearly similar.

It can be seen in Figure 2 that the reaction times in general show an increase as the number of choices increases. Analysis of the data using the F-ratio test for linearity of regression yielded a ratio that could have been obtained less than one time in a thousand on a chance basis. Thus it can be concluded that the means from Experiment II differ as a function of the number of choices involved. The mean reaction times that these subjects provided from Experiment I (solid line in Figure 2) were submitted to a similar analysis and they were found not to differ as a function of the number of choices.

FIGURE 2



Mean choice reaction times from Experiment II compared with those from Experiment I. N represents the number of subjects contributing reaction times to a given choice condition.

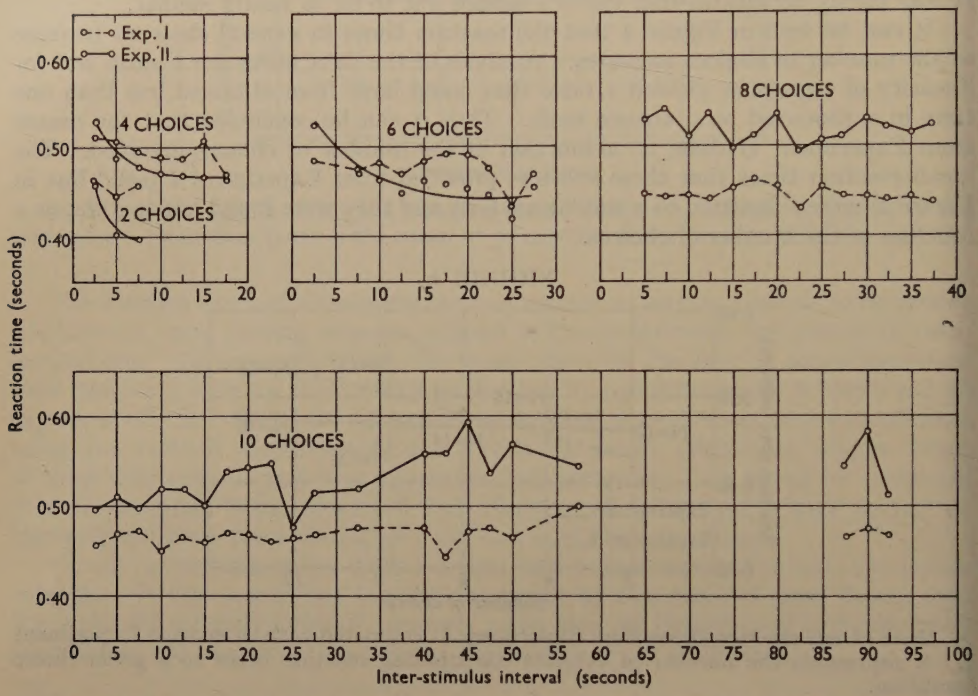
Once again the results from the male and female subjects were separated. As previously, the reaction times of the females were significantly slower than those of the males. It was also noted that the females showed a tendency to be more affected by the number of choices than did the males. As a matter of fact, a statistical check of the results from the male subjects for departure from zero slope yielded doubtful significance ($P = 0{\cdot}05$). A similar test on the data provided by the females resulted in a P of 0·001.

The results outlined so far from both experiments indicate that requiring a subject to respond to every one of several alternative stimuli is somehow different from requiring him to respond to only one stimulus out of several possible alternatives. Some further analyses were carried out on the data from both Experiment I and Experiment II in an attempt to gain some insight into the reasons for this difference.

In physical construction, the two experiments were identical, but in operation, it will be remembered, there was one major point of dissimilarity. Experiment I called for a response every 2·5 sec., whereas in Experiment II a response was required only when the key figure 8 appeared. Thus in the latter case, responses were generally random with the time interval between responses increasing on the average as the number of choices increased.

In order to determine whether the increasing inter-stimulus interval had contributed to the differences observed, an analysis of the reaction times to the key stimulus as a function of this interval was made. The reaction times from all subjects in Experiment II were used as well as the reaction times from Experiment I of those subjects who also served in Experiment II. The results from both experiments are separately plotted in Figure 3.

FIGURE 3



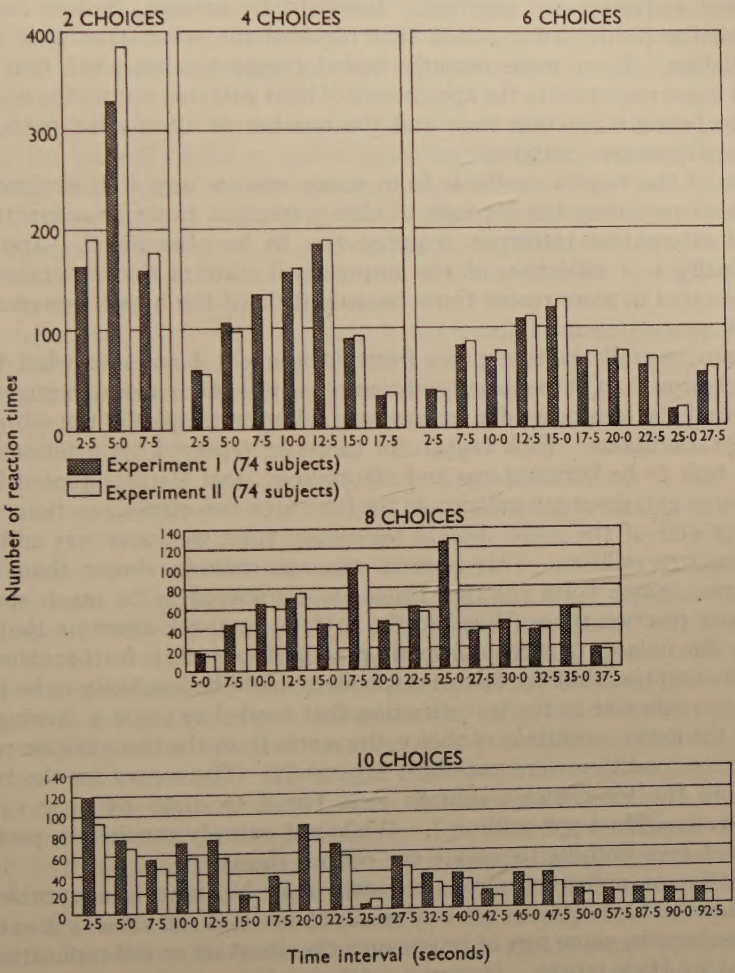
Mean choice reaction times from Experiment I and Experiment II plotted as a function of the inter-stimulus interval for all conditions of choice.

It is important to notice first that the data from Experiment II for 2, 4, and 6 choices show an unmistakable trend downward as the intervals between stimuli increase. This trend is not evident for 8 and 10 choices nor, apparently, for the data from Experiment I. Another thing to notice, and one that shows up grossly in Figure 2, is the fact that the reaction times for 2 choices and 4 choices are generally higher overall for Experiment I, whereas 8 and 10 choices yield higher reaction times for Experiment II. The six-choice condition seems to be the transition point although the bulk of the scores from Experiment I are higher.

It is clear that the actual distributions of intervals for any given choice condition are important in interpreting the data from Figure 3. These distributions for the two experiments are provided in Figure 4. Unfortunately this type of analysis was not anticipated when the experiments were designed, and there are thus too few data at certain of the intervals to be anything more than suggestive. The distributions of the intervals for the three lowest choice conditions are, however, fairly regular and have enough reaction times at most intervals to be dependable. For the two-choice condition, where the data for each interval used are abundant, the discrepancy

between the reaction times from the two experiments is marked. The same sort of discrepancy appears in the four-choice condition with the exception of the 17·5 sec. interval which is somewhat unreliable due to the fact that it contains only two reaction times for each subject. For the six-choice condition the discrepancy, while not marked, is nonetheless evident. A discussion of what is judged to be the significance of these points will be postponed for the moment.

FIGURE 4



The distributions of inter-stimulus intervals employed for all choice conditions in both experiments.

One further analysis of the data from Experiment II was made. It consisted of a comparison between the mean reaction times for the first 25 reactions and the last 25 reactions for each choice condition. The same comparison was made with the data from Experiment I and, it will be remembered, no significant differences were found. In this latter case, however, the reaction times from the second half of Experiment II were significantly higher than those from the first half ($P < 0{\cdot}01$).

The differences between the least squares fits to the data points were approximately 12 millisec. for 2 choices and 28 millisec. for 10 choices.

Discussion

The results of Experiment I indicate very convincingly that choice reaction times for the type of stimulus-response complex used do not increase as a function of the number of alternatives available to the responder. To put the matter in a more positive light, the results support the hypothesis that choice reaction times for practised responses are constant. Interestingly enough, Saslow has recently obtained similar results using a task that required the vocal naming of orally presented syllables. Even more recently Seibel (1959) has reported that extended practice of finger responses to the appearance of light patterns resulted in no consistent relationship between reaction time and the number of alternative patterns for as many as 30 alternative patterns.

In view of the results available from many sources now it is evident that the generalization regarding the increase of choice reaction times as a function of the number of alternative responses required has to be abandoned. Apparently it arose originally as a reflection of the unpractised state of the responders and has been perpetuated in more recent times because it fitted the hypothesis of man as an information processing machine.

The mean, overall reaction times from Experiment I are somewhat higher for the lower choices than those usually encountered in similar research suggesting the possibility that the results are due to slow responding on the part of the subjects in the lower choice conditions. Hick (1952), for example, reported his reaction time to a two-choice task to be between 250 and 260 millisec. and Merkel (quoted by Woodworth, 1938, p. 333) gives 316 millisec. as the time for a two-choice reaction. However, at the other end of the scale, Hick's ten-choice time was over 525 millisec. and Merkel's was 622 millisec. This is some 100–150 millisec. slower than the times reported here. Since voice reaction times are considered to be much slower than hand or finger reaction times (Woodworth, 1938, p. 329), the assertion that previous findings are due in large part to improperly practised subjects is further strengthened.

On the assumption that the fastest responders would be less likely to be influenced by any factors inherent in the test situation that tended to cause a slowing down of response to the lower conditions of choice, the scores from the three fastest responders for each choice condition were examined separately. The means for the two-choice condition and the ten-choice condition were found to differ by only 13 millisec. (365 millisec. as against 378 millisec.). While not entirely conclusive, perhaps, this result at least does nothing to negate the central thesis.

Fortunately, or unfortunately as the case may be, man is adaptable. Faced with an unfamiliar and complex sensory motor choice task, he attacks it as efficiently as he can, perhaps by some sort of progressive classification or self-replication process as suggested by Hick (1952). However, with repetition he improves until mastery of the task is complete. At this point he appears to have acquired a reflex. There is no longer an increasing relationship between the reaction time and the number of alternatives; that is to say, choice reaction time is a constant.

The complication provided by the results from Experiment II does not enhance the strength of the position adopted above. Yet, it must be remembered that from the subject's point of view these were two extremely different situations. The question that needs to be resolved, quite obviously, is what factors were operating in the second experiment that could have caused such differences as were found? To this writer, the most intriguing difference is the one that shows up for the three

lower conditions of choice in the time interval analysis (see Figure 3). When the subject had to respond only to the appearance of the key figure, as in Experiment II, his reaction times show a steady and consistent decline as the time interval between key stimuli increased. This suggests that the subject was operating consciously or unconsciously on an expectancy basis. An argument of this type would predict that the subject would not expect a key stimulus immediately following another key stimulus and thus his expectancy would be low, resulting in a higher reaction time. As the time since the last key stimulus increased, expectancy, based no doubt on experience with the task, would increase also, thus providing lowered reaction times. A look at the distributions of intervals encountered, shown in Figure 4, indicates that the subject would be thoroughly justified in adopting such a mode of behaviour.

Deese (1955) has proposed an expectancy hypothesis in an attempt to account for some of the facts of vigilance, and it seems worthwhile to consider this in relation to the results of the second experiment since there are some similarities between that experiment and a vigilance situation. Both tasks require that the subject be on the alert for a signal whose time of appearance is unpredictable for the most part. Furthermore, rate of signal appearance is a common variable in vigilance research that also appears in the reaction time study.

Two predictions about watch-keeping behaviour that stem from an expectancy hypothesis are the following: (*a*) the probability of detection of a signal should be low immediately following a signal and should improve as the inter-signal interval increases; (*b*) the overall probability of detecting a signal should be high for high signal rates and low for low signal rates. To further the proposed analogy between a vigilance task and the task provided by Experiment II it is only necessary to assume that probability of detection and response latency would behave similarly under similar conditions and, further, that the effects observed over long periods of time would also show up with the more abbreviated time intervals encountered in these experiments. The first of these assumptions has some experimental support. Jenkins (1954) provided his subjects in a vigilance experiment with a regular but unscored signal to which they had to respond by pressing a key. He found that latency of response was clearly associated with probability of detection. When signal rates were high and probability of detection was high, response latencies were low, and conversely, when signal rates were low and probability of detection was low, response latencies were high. The second assumption is merely a logical extension of Deese's expectancy hypothesis to include shorter time intervals.

Viewed in the light of the above discussion, the results from Experiment II are in fair accord with the predictions generated from an expectancy hypothesis. Certainly the three lowest conditions of choice show an unmistakable lowering of the reaction time as the inter-stimulus interval increases. Why the eight-choice and ten-choice conditions do not show this same effect is not known. It is perhaps reasonable to attribute the cause to the irregular time interval distributions at those two points and to the scarcity of data at many of the intervals. Further research is needed to resolve this question.

The data from this experiment conform also to the second prediction of the expectancy hypothesis in that the reaction times are low for high signal rates and high for low signal rates. In one other respect, too, these results are similar to those commonly encountered in vigilance research. Specifically, there is evidence of deterioration of performance with respect to the duration of the task. The reaction times for the second half of the experiment were significantly slower than for the first half. Furthermore, the low signal rates appear to be more affected than high

signal rates although it is not meant to imply that the small differences encountered here are anything more than suggestive.

In summary, then, it is suggested that the differences observed between these two experiments are a direct outcome of requiring a response to every stimulus in one case and to only one of *n* stimuli in the other. In the first instance a constant inter-stimulus interval and a constant stimulus rate resulted in constant overall latencies. Apparently the number of alternative responses available to the responder had no direct bearing on response latency. In the second instance there is strong, if not conclusive, evidence that the variables of inter-stimulus interval and stimulus rate operated to produce latencies that decreased as the interval increased and increased as the stimulus rate decreased. An expectancy hypothesis as outlined by Deese to interpret some of the facts of vigilance research is invoked and found to be reasonably adequate. However, any more positive conclusions must await future research using a variety of distributions of inter-stimulus intervals and stimulus rates.

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STUDIES IN OLFACTORY ACUITY

III. Relative Detectability of *n*-Aliphatic Acetates by the Rat

BY

D. G. MOULTON*

From the Department of Psychology, Stanford University, Stanford, California

The ability of black and albino rats to detect the first seven *n*-aliphatic acetates in the vapour phase has been investigated by use of an odour choice box. Both the median threshold and the slope of the probit regression line (which describes the relation between concentration and response) tend to decrease logarithmically in value as the series is ascended. The position of methyl acetate in these relations appears anomalous. When expressed as thermodynamic activities median thresholds for acetates of intermediate chain length are about equally stimulating, whilst short chain alcohols excite at decreasing activities with increasing chain length. The close similarity of these findings to those reported in the previous study on *n*-aliphatic alcohols supports the view that such relations may be of more general occurrence in olfaction in the rat.

The stimulus-response curve for each acetate studied shows one or more reversals in slope. It is suggested that certain of these are due to the presence of two or more olfactory receptor types one or more of which attain maximum response at lower concentrations of a given acetate than do the remainder. The asymptotes of the curves are reached in about 2–3 log units of concentration.

Introduction

There is growing evidence that in smell as in taste (Beidler, 1954; Dethier, 1956) the events leading to the excitation of the chemoreceptors are more probably biophysical than biochemical in nature. In particular, recent studies of olfaction in homologous series, in which behavioural and electrophysiological methods have been used, have emphasized the importance of properties such as solubility which alter progressively in value within a series (e.g. Dethier and Yost, 1952; Ottoson, 1958). Thus in a previous report, describing the responses of rats to alcohols, it was found that detectability at median threshold rises logarithmically with increasing chain length (Moulton and Eayrs, 1960). It can also be argued that the tendency noted by these and other workers for intermediate members of certain homologous series to be equally effective odorants at about equal thermodynamic activities is further evidence that the dominant process in the overall effect is physical.

However, since many physical properties vary in a similar manner as chain length is expanded, consideration of their relations to odour intensity in a single series will throw limited light on their relative significance for olfaction. As the important studies of Dethier and Chadwick on tarsal chemoreception in the blowfly (see Dethier, 1956) have clearly illustrated, information of this kind is more likely to be derived by an analysis of such relations for a number of series in which different substitutions in the same carbon skeleton do not alter all properties in the same way. Evidence which would permit such an analysis is scarce and relates to species other than the rat. It is therefore one aim of this study to provide data on the detectability by the rat of a further series of homologous odorants.

A second aim concerns the finding of the previous report that in alcohols the slopes of the probit regression lines (which describe relations between concentration and response) decrease logarithmically in value as the series is ascended. Since this

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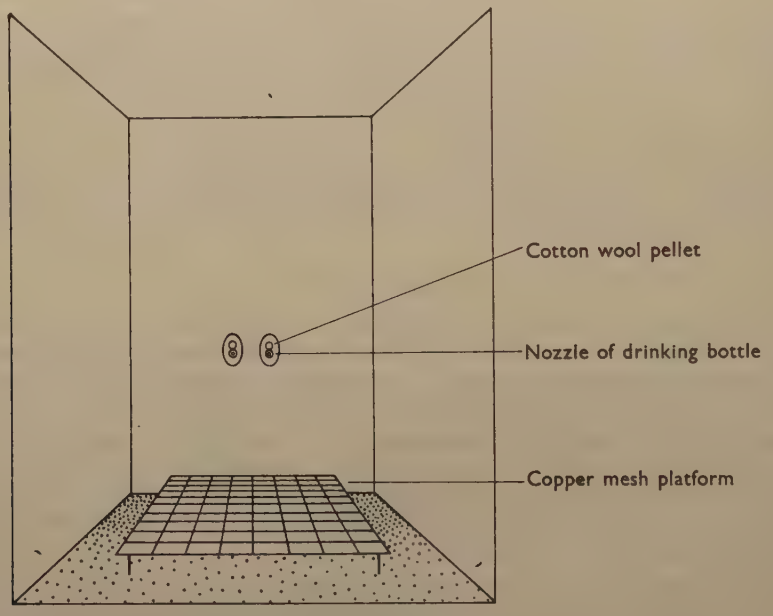
implies that there is a progressive change in the pattern of detectability as the concentrations are increased and therefore may explain the apparently conflicting claims in the literature about the pattern of detectability in homologous series, it is important to know whether this trend is unique to the alcohols or represents a more general phenomenon. The following study has therefore been designed to provide data over a wide range of the stimulus-response relations in the first seven *n*-aliphatic acetates.

Materials and Methods

Apparatus

All rats were initially trained in the circular choice apparatus described by Eayrs and Moulton (1960), but in an attempt to reduce the time required to test rats a modified form of the rectangular choice box (also described by Eayrs and Moulton) was used in definitive trials (Fig. 1.) This version has three hardboard walls and a plastic floor

FIGURE 1



Odour choice box (for description, see text).

supporting a copper mesh platform. Both floor and platform can be removed for cleaning. Two bottles are hung behind the wall facing the open side of the box so that their spouts protrude through two apertures above the platform. These spouts are identical to those used in the circular choice apparatus, each bearing a holder into which can be fitted a detachable nylon capsule. A cotton wool pellet soaked with either the odorant or the control solution is placed in each capsule. Both tubes are connected by wires and clips to an "on-off-on" switch and hence to one pole of a shocking device whilst the other pole is connected to the platform on which the rat stands. In this way either spout can be charged so that a rat drinking from it receives a shock. The position of the negative capsule, containing the odorous pellet is varied according to a randomly determined sequence so that a given dilution of a test compound appears as often on the right-hand position as on the left. Other details of procedure and scoring of trials were as described by Eayrs and Moulton (1960).

Animals

In the main experiment 42 black and albino male rats, taken from a stock initially derived by crossing black and albino strains of the Norway rat (*Rattus norvegicus*, Berkenhout), were used. All rats had been trained to detect members in the alcohol series and

18 of them were subjects in the previous study. At the beginning of the experiment they varied in age from about 11–13 months.

In order to determine the ease with which experimentally naïve rats could be trained, a group of 5 young albino and black rats were also tested in the odour choice box after they had been given two nights training on a modified circular choice apparatus. The test odorant was a concentrated solution of butyl bromide and the rats were not used in investigating the detectability of *n*-aliphatic acetates.

Odorants

All reagents used were obtained from the Eastman Kodak Company. The acetates were diluted with propylene glycol by successive factors of two, essentially according to the method described by Beck, Kruger and Calabresi (1954), to form a geometric series of dilutions.

Experimental design

In an attempt to minimize the influence of day to day fluctuations in performance the series of dilutions chosen for each acetate was presented to the rat in accordance with a balanced incomplete block design. Thus nine dilutions were tested in a series of twelve blocks, each of which consisted of three dilutions presented in a randomly determined manner. This allowed four replications of each dilution to be made. 42 rats were tested on each of the acetates which were presented in the order: amyl, ethyl, butyl, methyl, propyl, heptyl, hexyl. The appearance of anomalies in the stimulus-response curves of these acetates suggested that additional data would be of value. Replications for three acetates were therefore made using identical procedures and the same number of rats, but employing fresh sets of dilutions.

Treatment of data

The results were subjected to probit analysis (Finney, 1952) and values for median threshold response were determined as molar concentrations in solution. The procedure used for estimating the concentration of the alcohol vapour and the thermodynamic activities corresponding to these values were as described by Moulton and Eayrs (1960).

Cheesman and Mayne (1953) have pointed out that the application of probit analysis to data such as the present, results in anomalies which have the effect of setting fiducial limits at artificially low values. For this reason fiducial limits have not been used in assessing the results.

Results

Technique

Rats which had previously been trained and tested in the circular choice apparatus rapidly learned the discrimination when transferred to the odour choice box, whilst the group of 5 experimentally naïve rats reached the criterion of 10 consecutive correct responses in 38 trials or less. Fully trained rats could be tested much more rapidly in the odour choice box than in the circular choice apparatus.

TABLE I

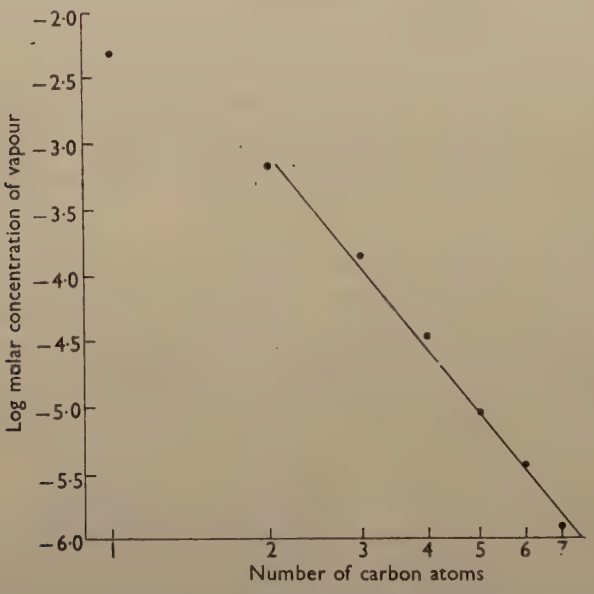
Detectability of *n*-Aliphatic Acetates in the Vapour Phase by 42 Rats. Each Rat Received 36 Trials on Each Acetate

Acetate	*Slope of regression line*	*Log molar concentration of solution*	*Log molar concentration of vapour*	*Log activity*
Methyl ..	1·037	0·7720	−2·3267	−0·3676
Ethyl ..	1·396	0·2900	−3·1891	−0·8348
Propyl ..	0·776	0·0030	−3·8536	−1·1206
Butyl ..	1·025	−0·1410	−4·4746	−1·2634
Amyl ..	0·534	−0·1850	−5·0429	−1·3047
Hexyl ..	0·601	−0·1815	−5·4437	−1·2970
Heptyl ..	0·390	−0·2337	−5·8623	−1·2459

Relative detectability of n-aliphatic acetates

It is apparent from the results given in Column 4 of Table I that the median threshold decreases logarithmically as chain length is expanded and if the value for methyl acetate is excluded there is clearly a very high degree of correlation. This fact is emphasized in Figure 2 in which molar concentrations at median threshold are plotted against the log of the number of carbon atoms in the alkyl group of the molecule. The values of the slopes of the probit regression lines given in Table I also show a linear relation to chain length and although the correlation is less striking the tendency for slopes to decrease logarithmically in value as the series is ascended is significant at the two per cent. level. This relation implies that the detectability pattern of the acetates varies with concentration.

FIGURE 2



Relation of median threshold concentration of an homologous series of acetates, expressed as log molar concentration of vapour, to the number of carbon atoms in the alkyl group of the molecule.

Since there are grounds for expecting the first member of a series to show anomalous behaviour the value for methyl acetate has been excluded from the computation of the regression line shown in Figure 2 (cf. Moulton and Eayrs, 1960).

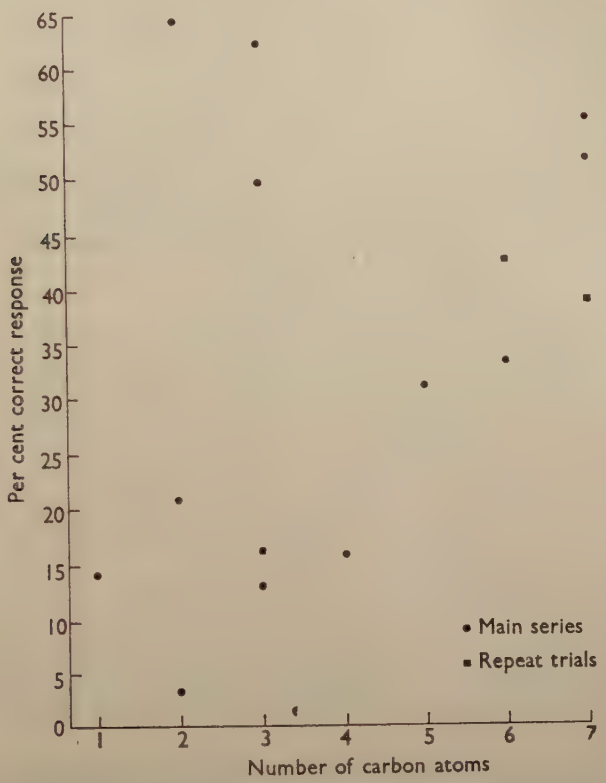
Anomalies in the stimulus-response curves for the acetates

In the stimulus-response curves of the acetates studied there is a series of reversals in slope. An example of this is seen in the data for amyl acetate plotted in Figure 5, which is more conveniently placed in the discussion. If the percentage correct score corresponding to the lowest point in the trough of each reversal is plotted against chain length as is done in Figure 3, the distribution of these reversals in relation to their position on each curve becomes apparent. (A similar pattern results when the peak instead of the trough of a reversal is used as a criterion.) From this it appears that in members above ethyl acetate only one reversal is consistently present on successive replications and this shows a tendency to occur at progressively higher

response levels as the series is ascended. In fact there is a significant correlation between the position of these reversals on the curve and chain length ($p < 0{\cdot}001$).

The asymptotes of the curves are reached in about 2–3 log units of molar concentration.

FIGURE 3



The position of reversals of slope on the stimulus-response curves of an homologous series of acetates. The percentage correct score corresponding to the trough of each reversal is plotted as a function of the number of carbon atoms in the alkyl group of the molecule.

Discussion

Technique

It has been shown that the circular choice apparatus can be used effectively to train and test rats in an odour choice situation (Eayrs and Moulton, 1960). However, when an extended series of trials is undertaken the routine cleaning of the apparatus and the test procedure becomes laborious and time consuming. The present results indicate that these difficulties can be greatly reduced and the advantages of automatic training largely retained if preliminary training on the circular choice apparatus is followed by use of the modified odour choice box for routine testing. Thus the testing procedure is much simplified while the discrimination is learned rapidly and without the appearance of behavioural disturbances such as occurred during trials with the rectangular choice box (Eayrs and Moulton, 1960). Further, the box is easily incorporated into an olfactometer utilizing air dilution of the odorant (Moulton, unpublished).

That this technique is also effective in controlling the influence of non-olfactory

cues is seen in the fact that response declines to chance level with progressively increasing dilutions of the test odorant.

The detectability of the n-*aliphatic acetates*

Although few systematic studies of the olfactory stimulating efficiency of acetates have been made, the results, in contrast to those relating to the alcohol and fatty acid series, are in broad agreement. Thus the responsiveness of flies to these compounds (e.g. Cook, 1926; Hughes, 1957) and the magnitude of the slow potentials evoked in the olfactory epithelium of the frog by the first four members of the series (Ottoson, 1958) increase as carbon chain length is expanded. Similarly both Allison and Katz (1919) and Jones (1955) report lower human olfactory thresholds for amyl than for methyl acetate. What appears to be the only conflicting evidence derives from a study by Beck, Kruger and Calabresi (1954) who claim that in man odour intensity in acetates with 6–12 carbon atoms decreases with increasing chain length. However, it is possible that if corrections for vapour pressure were applied to this data the trend would be reversed.

With this exception, then, the present conclusion that the detectability of acetates increases logarithmically as chain length is expanded accords with existing evidence for this series, particularly with that obtained by Hughes on the tsetsefly. There is also a striking similarity between the current results and those of the previous study on alcohols. Thus although the slopes of the lines describing the regressions of concentration at median threshold on chain length differ significantly for the two series ($p < 0{\cdot}001$) the disparity in regression coefficients is nevertheless small (0·09). There is also a significant tendency for the slopes of probit regression lines to decrease in value as both series are ascended.

This conformity suggests that such relations may be of general occurrence in olfaction in the rat, at least in so far as homologous series of aliphatic compounds are concerned. It also confirms the finding that the detectability pattern can be a function of concentration. As was previously suggested this may explain why some workers have found a progressive rise, others an oscillation in odour intensity as a series is ascended.

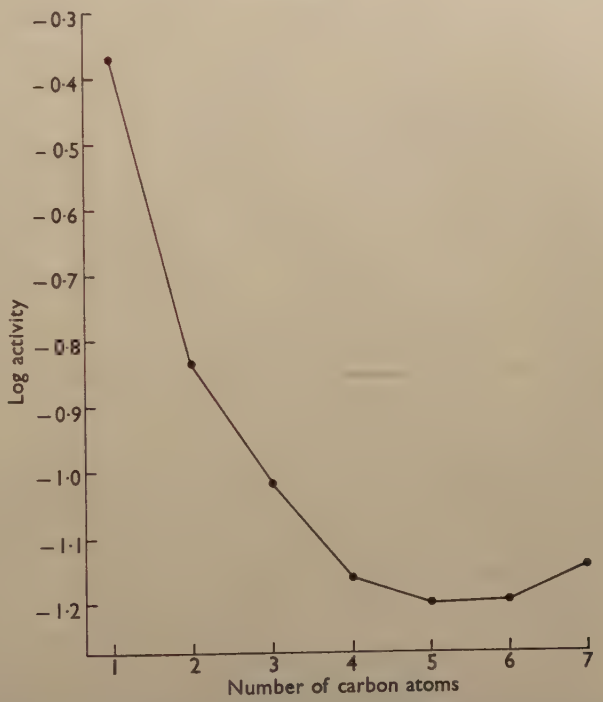
When median threshold concentrations of acetates expressed in molar units (Table I) are compared with the equivalent values for alcohols given in the previous study, the acetates appear to be considerably less stimulating. This is an unexpected result if such properties as solubility and vapour pressure are held to exert any significant influence over detectability. However, this comparison must be viewed with caution since the conditions of the two experiments differed in several respects. In particular, the fact that animals tested on acetates were about 7–9 months older than those used on alcohols may be important. The rat is susceptible to rhinitic infections which generate pathological changes (Kelemen and Sargent, 1946; cf. Clark, 1956), and in one study the volume of the olfactory bulb in male albino rats was found to have decreased by more than 50 per cent. during the second year of life (Smith, 1935). Thus although little is known about the way in which olfactory acuity alters with time, its deterioration in this species at least, seems likely to be marked beyond a certain age. Because of this factor limited value may lie in comparing olfactory thresholds derived from subjects of dissimilar ages.

Analysis of results in terms of thermodynamic activity units

If olfaction in the acetates involves an equilibrium process, median threshold values might be expected to be about equally stimulating when expressed in thermodynamic activity units (see Moulton and Eayrs, 1960). In Figure 4 these values are

plotted against chain length and it is clear that in members containing 4–7 carbon atoms this is in fact the case. However the first four members, although occupying a narrow range of activities, stimulate at decreasing levels as the series is ascended. The pattern is thus closely similar to that derived from the majority of recent studies on olfaction in homologous series, whether in blowflies (Dethier, 1954; Dethier and Yost, 1952), the dog (Moulton, Ashton and Eayrs, 1960), the frog (Ottoson, 1958) or the rat (Moulton and Eayrs, 1960). Whilst the existence of conflicting data relating chiefly to man (see Moulton and Eayrs, 1960) stresses the need for further studies, the weight of evidence now tends to favour the view that in several homologous series olfaction in members of intermediate chain length involves the establishment of an equilibrium, and it can therefore be argued that physical rather than chemical processes control olfaction in these members. Olfaction in the remaining members either does not involve an equilibrium process or it is being obscured by some factor, such as might control the amount of an odorant reaching the olfactory surface.

FIGURE 4



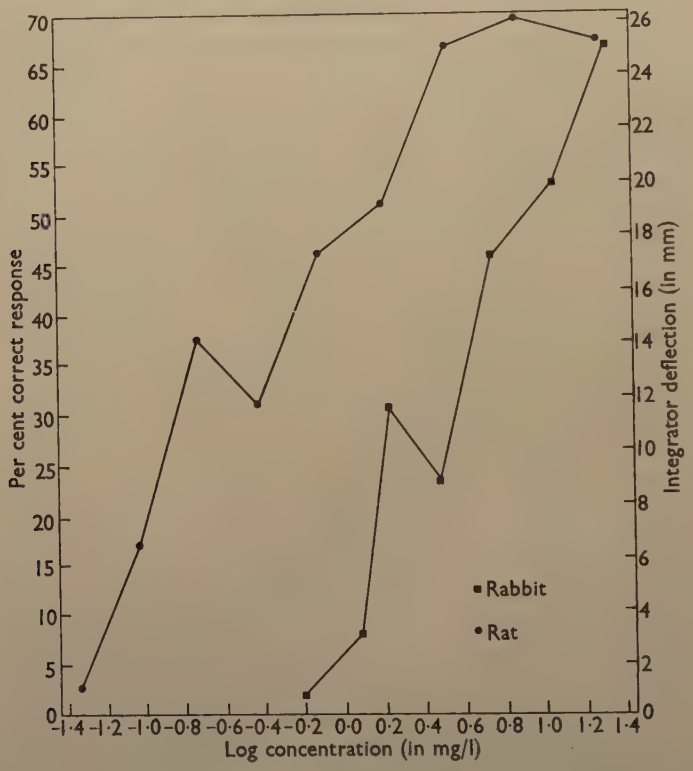
Relation of median threshold concentrations, expressed as thermodynamic activities, to the number of carbon atoms in the alkyl group in an homologous series of acetates.

The stimulus-response curves for acetates

(*a*) *The nature of the anomalies present in the curves.* The most surprising feature of the results is the appearance in the stimulus-response curves of a series of pronounced reversals in slope which bear a linear relation to chain length. Although two or three reversals occur in some curves replications indicate that only one reversal in each acetate above ethyl is related to this trend, and it varies little in appearance throughout the series. Clearly the nature and possible cause of these anomalies are worth considering.

An example of a reversal can be seen in Figure 5 in which the stimulus-respons curve for amyl acetate, obtained in the present study, is compared with that derive by Mozell (1958) from recordings of the electrical activity in the olfactory bull of the rabbit. Although the extreme degree of flattening at the upper margin of th left hand curve, and the low response level at which it occurs, are exceptional, it is, i its general form, representative of data obtained for this series in the present study Similarly the right hand curve in Figure 5 is considered by Mozell to be typical o his data for amyl acetate, although variations occurred, depending partly on th position of the recording electrode in the bulb. In comparing these two curves it i of interest to note the similarity both in the general shape, and in the position an form of the reversal. This suggests that the anomaly in the present data for amy acetate, and thus probably in the remaining acetates above propyl were studied, represent olfactory events.

FIGURE 5



Relation between concentration of vapour of amyl acetate and intensity of response The behavioural responses of rats, expressed as percentage correct scores, are compare with data for the rabbit derived by Mozell (1958) from recordings of the electrical activit of the olfactory bulb.

However, in the curves for ethyl and propyl acetate, as in those for alcohol containing 5–11 carbon atoms (Moulton and Eayrs, 1960), anomalies occur which ar associated with high concentrations of the stimulus. As we have already indicate these are of more doubtful significance since at this level many odorants stimulat non-olfactory receptors and free nerve endings in the nasal mucosa (Parker an

Stabler, 1913; Katz and Talbert, 1930). Such effects may be responsible for Bachem's (1956) claim, which is clearly contradicted by the data of Figure 5, that the sensory curve for olfaction is positively curved throughout its length and exhibits no saturation effect. It may not be so unique among sensory curves as he suggests.

However, the cause of the reversals which conform to the main trend remain to be considered. In the following section a possible explanation will be advanced.

(*b*) *A possible cause of certain anomalies present in the curves.* In recording the electrical activity of the olfactory bulb of the rabbit, Adrian (1953) noted that amyl acetate stimulate both large and small spike units, and he suggested that different groups of receptors were responding. It is thus possible that at low concentrations acetates excite two or more receptor types. As the concentration of a given acetate is increased, however, the firing of one or more receptor types may reach a maximum and then decline, whilst remaining groups continue to show increasing response with increasing concentration. This is consistent with Mullins' (1955) suggestion that some compounds may narcotize certain receptors more easily than others, and could account for a reversal in the stimulus-response curve. However it is also possible that some substances may be bound or adsorbed on to (cf. Beidler, 1954; Moncrieff, 1957), penetrate and accumulate in (cf. Davies, 1953), or otherwise saturate the surface of certain receptors, receptor sites, or phases associated with them, more easily than others. Such effects could lead to a depletion in response but can less readily account for a reversal as opposed to a flattening of the curve.

This may account not only for certain anomalies in the present results and in the curves for several other odorants (Allison and Katz, 1919; Katz and Talbert, 1930) but also for the common observation that the quality of many odours changes on dilution (e.g. Passy, 1892). However, until more evidence is available further speculation on this topic seems premature.

(*c*) *Discrepancies between behavioural and electrophysiological data for amyl acetate.* A further point which stems from a comparison of the curves shown in Figure 5 is that the behavioural data cover a wider range of concentrations than do the electrophysiological data, the discrepancy being about 0·5 log units. Although the value of this comparison is restricted by differences in such factors as the species studied, the number of subjects employed and the use of anaesthetics, a disparity in this direction might nevertheless be expected. The behavioural responses of the intact animal to an odour draw on the firing of a larger pool of olfactory receptors than do those recorded electrophysiologically from one point in the olfactory system, and the probability that receptors of unusual sensitivity are represented will therefore be enhanced. This could account for the fact that the greater part of the discrepancy occurs in the lower segment of the curves.

For these and similar reasons (see, for example, Barlow, FitzHugh, and Kuffler, 1957) it is to be expected that behavioural thresholds should be lower than those obtained electrophysiologically, and it cannot, of course, be implied from Figure 5 that the rat has a lower threshold for amyl acetate than the rabbit. In fact these data suggest that the threshold for the rabbit is about twenty times higher than that of the rat, whereas the equivalent data for the absolute threshold for vision in the cat show an even greater difference. Thus the most sensitive unit in the retina from which Barlow, FitzHugh and Kuffler (1957) recorded, had a threshold which was fifty times higher than that derived behaviourally by Gunter (1951).

In the acetates studied here the asymptotes of the stimulus-response curves are reached in about 2–3 log units of concentration. Mozell's data for a number of odorants show an even smaller range of 1–1·5 log units and he has pointed out that in other senses an increase of 4, 5 or 6 log units of intensity is required (see also

Holway and Pratt, 1936; Bachem, 1956). However, there is considerable variation in this figure for olfaction and for some compounds at least the intensity range may conform more closely to that common in vision and audition (cf. Katz and Talbert, 1930; Moulton, Ashton and Eayrs, 1960).

Conclusions

The present results have confirmed the general findings of the previous report, thus reinforcing the view that the physical properties of a molecule play a dominant role in determining olfactory stimulating efficiency within homologous series and again emphasizing the striking similarity of olfactory responses in the rat and certain species of insects. It is now clear that the rat is a valuable subject for quantitative behavioural studies of olfactory response.

It has also been shown that the stimulus-response curve for olfaction, which has received scant attention in the past, may be more complex in form than is generally assumed and deserves closer study. It may even provide a means of investigating the behaviour of different olfactory receptor types.

My thanks are due to Dr. J. M. Warren for reading the manuscript of this paper and for his assistance in obtaining facilities for the work.

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BROADBENT'S FILTER THEORY: POSTULATE H AND THE PROBLEM OF SWITCHING TIME

BY

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Groups of digits were presented binaurally and dichotically to subjects who were asked to recall them. Different rates and patterns of presentation were used, the design being based on that of Broadbent (1954). Broadbent's findings in this field were confirmed. If subjects are presented with simultaneous pairs of digits at a rate of 2 pairs per second dichotically, they cannot recall them if they are asked to alternate between the ears. If however the presentation is staggered, so that although the rate is constant the signals do not overlap, subjects can recall alternately from the two ears. It is suggested that these findings are better interpreted as an interference effect, not a rate effect. Criticism is offered of similar designs to measure "switching rate." Criticism is also offered of Broadbent's estimate of "perception time" in such experiments. An analysis of the quantity and type of errors made by subjects is given, which suggests that Broadbent's theory of a short term store on the peripheral side of a selective filter is in need of revision.

Introduction

Broadbent (1954) presented pairs of digits simultaneously and dichotically to subjects. The signals came in groups of three pairs, and subjects were required to recall the digits either successively (LLLRRR) or by alternating between the ears (LRLRLR). Broadbent found that the latter was only possible providing that the rate of presentation was not greater than one pair every 1½ sec., although if recall were successive the subjects could recall accurately even when the rate was as high as 2 pairs per sec. He interpreted these results to mean that at the faster rates the subject could not switch from ear to ear fast enough, and that the only successful way to deal with the situation was to listen to one ear while the material arriving at the other ear was retained in a short term store on the peripheral side of what he called a "Filter Mechanism."

Now it could be argued that the above experiment is not a fair test of a subject's ability to switch between signals: for what in fact a subject is doing in such a situation is to switch between the signal he hears and a *trace* of the other, not the other signal itself. It seemed that it should give a more genuine measurement of the subject's performance in a switching task if the signals came not in simultaneous pairs, but staggered, so that they never actually overlapped. The following experiment was devised.

Experiment I

Method

Lists of six digits were prepared, and presented to the subjects in groups of ten lists. There is a 10 sec. pause between each group of six during which the subject had to recall. A Brennell Mk. IV two-channel tape recorder was used to present the stimuli to the subjects through headphones, which were Brown moving coil, low impedance type. Subjects were asked to recall the digits either in any order they liked (free recall), or all the digits from one ear followed by all those from the other (successive recall), or by switching from ear to ear (alternate recall). A binaural condition was also used, in which cases the stimuli were obtained by passing the above dichotic lists through a mixing box and then feeding them to both headphones. The free recall conditions were given before any of the other conditions so that all subjects were well practised. The dichotic conditions of presentation were as follows: simultaneous (= = =) at 2 signals/ear/sec.; staggered (−·_ −·_ −·_) at 2 signals/ear/sec., and slow overlapping (——— ——— ———) at

1½ signals/ear/sec. Half the subjects received the staggered before the alternate stimuli and half vice versa. Sixteen subjects were used, undergraduates and research workers between the ages of 18 and 30.

Results and Discussion

The form of presentation and recall, and the results, are shown in Table I and Figure 1.

TABLE I

Procedure and Results of Dichotic Listening Experiment

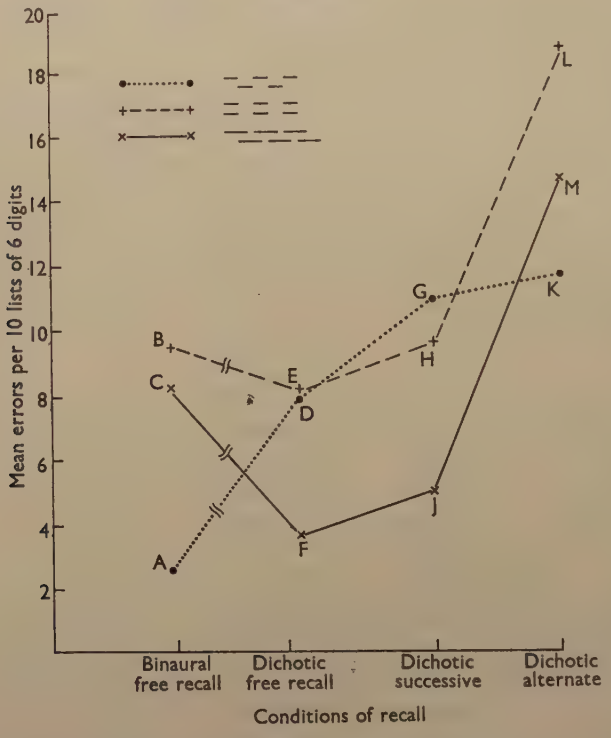
Presentation	*Recall*	*Rate*	*Mean errors per 10 6-digit lists*	*S.E.*	*Condition*
successive	free	4/ear/sec.	3·0	0·7	A
simultaneous	free	4/ear/sec.	9·9	1·3	B
slow overlapping	free	3/ear/sec.	8·0	1·0	C
staggered	free	2/ear/sec.	7·6	2·9	D
simultaneous	free	2/ear/sec.	7·7	1·7	E
slow overlapping	free	1½/ear/sec.	3·1	0·8	F
staggered	successive	2/ear/sec.	11·3	2·0	G
simultaneous	successive	2/ear/sec.	10·0	1·7	H
slow overlapping	successive	1½/ear/sec.	4·9	1·8	J
staggered	alternate	2/ear/sec.	13·5	1·9	K
simultaneous	alternate	2/ear/sec.	19·9	2·1	L
slow overlapping	alternate	1½/ear/sec.	14·6	2·3	M

There is a highly significant difference between successive and alternate recall for simultaneous digits, as Broadbent found: L > H ($p < 0.001$ by t test). But there is no significant difference in the case of the signals which were presented in a staggered order in the comparable recall conditions ($p > 0.1$). That is, if the signals do not overlap, they can be recalled perfectly alternately even at a rate of 2/ear/sec.—a rate far greater than the limiting rate found by Broadbent for simultaneous digits. It might be thought that staggered signals were inherently easier to perceive than simultaneous ones because of the signals at either end which are "unencumbered" by competing ones; or perhaps that in the simultaneous case the time taken to switch would allow the other half of each pair to fade slightly, but that there is no such advantage is shown by the lack of difference between conditions G and H.

It appears therefore as if the limit on the alternate recall of simultaneous digits is one of interference rather than rate. But rate of presentation does indeed play some part. There is a remarkably constant difference between E, D and F; G, H

and J; and L and M. The only factor common to all these situations is a difference in rate of presentation, and moreover the difference itself is the same in each case. So we may conclude that the difference in performance due to rate of presentation is shown not by the fact that the subjects cannot recall alternately in the simultaneous presentation condition, but by the fact that the 1½/ sec. line is everywhere (in the dichotic situations) lower than the 2/sec. line.

FIGURE 1



Broadbent suggested that the way to estimate switching time was by subtracting the time for two perceptions from the total time for two perceptions plus two switches, the latter being calculated from the subjects' inability to respond to simultaneous stimuli by alternate recall if the stimuli arrived more rapidly than one pair every one and a half sec. He assumed that a perception took about ½ sec. But if we consider condition A in the present experiment, where a binaural condition made by mixing the staggered diochotic stimuli, we have a condition in which the subject was receiving signals at the rate of 4 per sec., and yet the error score is the lowest of the entire experiment. This being so, it is clear that the time taken to perceive a digit must be very much less than 250 millisec., for if it even approached that value we would expect a much higher error score in Condition A. In an unpublished experiment on audioverbal reaction times the writer investigated how much of the signal was needed by the subject to identify a digit, if he knew that the stimulus would be one of the digits "nought" to "nine." An electronic device was used to cut off the signal a predetermined time after it began, and the result showed that there was almost 100 per cent. correct recognition when as little as 30–40 millisec of the signal—whose total length was about 200 millesec. or more—was heard by the

subjects. Now this cannot be used as directly relevant data in the present case, for here the whole of the signal is in fact present, and we do not know how much of it the subject listens to. But it does raise the question of how long a "perception" takes. In the opinion of the writer this question is not one to which it is possible to give an answer. Different sorts of signals may require very different amounts of time to "give rise to a perception," and it is more than likely that had Broadbent used signals of shorter duration than spoken digits he would have obtained data suggesting that the switching time was very much shorter.

Another possible interpretation of the difference between Conditions K and L is this. It may be the case that when signals overlap in time the subject does have to switch from ear to ear; but when they are staggered he listens to both ears simultaneously—as it were "sitting in the middle of his head" instead of "rushing backwards and forwards between his ears." If so, then providing that the signals are staggered, there will never be a decrease in performance with alternating recall unless the signals arrive so rapidly that they would be misheard even in a binaural situation; the suggestion in fact is that even though the stimuli are presented dichotically the subject treats the situation as a binaural one unless they overlap and interfere with one another. In this case the difference between simultaneous and staggered conditions reflects a change in the "strategy" adopted by the brain to deal with different stimulus conditions, but again is not a matter of rate so much as interference. As an alternative to Broadbent's model, for example, one might suggest that when the signals overlap, rather than one set being held up while the other is dealt with, the following happens. The two sets are passed through the filter together but are stored in separate stores according to the source from which they came. During recall they are reproduced store by store.

The above discussion—on the possibility of the subject "sitting in the middle of his head" in a dichotic listening situation—draws attention to another point, one of general interest in any attempt to measure "switching time" by methods such as have recently been used. In none of the experiments so far done on this topic (whether Broadbent's or those of the writer), nor in those which are often invoked in discussions (such as the speech switching experiments of Cherry and Taylor (1954)), is there any guarantee that the subjects *must* have been switching. It is impossible to ensure that a subject is "switching" merely by sending stimuli along what appear to be two separate channels. Whatever our theoretical model may be there is always the possibility that the subject is listening to both ears at once. The following analogy will point the moral. If you are trying to identify people emerging from a house by two doors, there are two strategies at least open to you. Either you can glance rapidly from door to door, in which case the limits on your performance will be due to the rate at which you switch your gaze; or you can steadily fixate a point midway between them, in which case the limiting factor on your accuracy will be the acuity of peripheral vision. But it is impossible to decide which of these things is being done merely by sending people out through the doors at different rates and in different orders. This sort of criticism can be made of all current methods in this field, and it remains to be seen whether ingenuity will triumph over the difficulty or whether we must await a physiological attack on the problem.

We have shown that in some ways an "interference" rather than a "rate" model provides a better account of the limits of performance in these studies. It has also been mentioned in passing that an alternative model without a short term store on the peripheral side of the filter could be designed. Is there any evidence which in fact casts doubt upon this part of Broadbent's model? In his 1954 paper on the perception of simultaneous pairs of digits there is one important score which is not given;

that is the effect of rate on successive recall following simultaneous presentation (LLLRRR following = = =). The following experiment was designed to furnish this data.

Experiment II

Method

Pairs of simultaneous dichotic digits were presented as in Experiment I. Three speeds of presentation were used, 2 pairs/sec., 1 pair/sec., and 1 pair every 2 sec. Subjects were asked to recall successively, that is, all the numbers from one ear followed by all those from the other, starting with whichever ear they pleased. A practice run of ten groups of 3 pairs of digits was given before the experiment started. Half the subjects received the fast condition followed by the medium and the slow speeds, half the reverse. 10 subjects, undergraduates of both sexes, were used.

Results and Discussion

The results are shown in Table II.

TABLE II

The Effect of Rate of Presentation of Simultaneous Digits on Successive Recall

Rate of presentation					
Fast 2 pairs/sec.		*Medium 1 pair/sec.*		*Slow 1 pair/2 sec.*	
Mean	*s.e.*	*Mean*	*s.e.*	*Mean*	*s.e.*
5·55	1·06	4·7	0·89	3·7	1·21

Means are the mean errors per 10 groups of 6 digits for 10 subjects (pooled data).

In his book *Perception and Communication*, Broadbent (1958) stated that the time for which digits could remain in the peripheral store without fading was "of the order of seconds." He has more recently said (Broadbent, 1959), that by this he meant "about a second." Now if this is so, then those digits which have to be stored for more than a second will tend to fade, and we can expect that if the subject must store digits for so long then he will make mistakes during recall. If pairs of digits are presented at the rate of one pair per second, the total time which it takes to present three pairs is rather more than a second, from the beginning of the first to the end of the last (quite apart from any hypothetical "switching time"). So if the subject were letting through, say, the right ear message and storing the left ear message, we would expect the left ear signals to show some signs of deterioration. If we interpret the time constant of "about 1 sec." fairly strictly, we would expect about 1 digit per group of three to show marked signs of deterioration, and moreover, it should be the first one of the "stored" message which shows the greatest deterioration, for this is the one which has been stored the longest. Similarly, if the rate of presentation is greater than this the errors on recall of the "stored" message should be smaller, and those of a slower rate of presentation greater. In the present study the results of the slowest rate of presentation are difficult to interpret, for at a rate of presentation of 1 pair of signals every 2 sec. the subject has time to rehearse what he has already heard. But the data on the other two conditions is clear: the difference, while not being significant, is if anything in the opposite direction to that required by Broadbent's theoretical model. Even if we interpret the time constant of "1 sec." rather

liberally it is strange that the 1 pair per sec. rate of presentation does not show a greater number of errors than the faster rate.

Furthermore, in Table III is given the distribution of errors among the six possible

TABLE III

RELATION BETWEEN ERRORS AND POSITION OF RECALL OF DIGITS IN EXPERIMENT II

Position in recall	*1st*	*2nd*	*3rd*	*4th*	*5th*	*6th*
Speed of presentation						
Fast	5	9	7	12	15	7
Medium..	9	7	4	13	10	4
Slow	2	4	5	8	10	8
F + M + S	16	20	16	33	35	19

Pooled data, 10 subjects.

positions which a digit can occupy during recall. It will be seen that in the overall score it is actually the middle one of the "stored" digits, not the first, which shows the greatest number of errors, although none of these differences (between the 1st, 2nd, and 3rd positions of the "stored" digits) are significant.

Again, the actual total number of errors seems to be too low for the requirements of Broadbent's theory: the average error for each subject in the 1 pair per sec. group works out at only 1·3 digits *per* 10 *groups of* 3 *pairs*, and it would have been lower than this if it had not been for the relatively large scores of one or two subjects. As we said above, it would be expected that the average error for the first of the "stored" digits should be nearer to 10 per group of 10 lists than 1. And in fact if the estimate of "about 1 second" is anything like accurate, *all* the errors are too few.

TABLE IV

ERRORS OF TRANSPOSITION IN RECALL AND ERRORS OF OTHER TYPES IN EXPERIMENT II

Errors of transposition	*Other errors*
44	58

Data from 10 subjects, pooled Fast and Medium conditions.

All the above considerations seem to cast doubt on the existence of a short term memory store on the peripheral side of the filter, in which the input from one ear is held while that from the other is allowed through into the perceptual system. But it does not do more than cast doubt, for the arguments turn on the timing relationships between the signals, and as been pointed out there are two places where there is a great deal of vagueness: firstly, we do not (and probably cannot) know how much of the signals the subject utilizes in identifying which signal he has perceived—that is the "perception time"; and secondly, without knowing more about the exact value of the time constant of the store, we cannot really make predictions about how many digits should be lost in various situations. Indeed, with regard to this latter point, while the inherent vagueness of the postulate remains, it is probably impossible to falsify, although the experiments here described cast severe doubt upon it since neither the number nor the distribution of the errors is what would be predicted.

Finally, in Table IV, there are given the relative number of two different kinds of errors which subjects make during recall. The number of errors which seem to be transpositions between the two ears is compared with the number of errors which seem to be due to mistakes or omissions (that is, if the stimulus was $\begin{matrix}1&2&3\\4&5&6\end{matrix}$, mistakes of the kind 423, 156 are compared with, say, 923, 456 or 123, –56). It will be seen that the number of mistakes due to transposition is $\frac{3}{4}$ of the number due to all other causes, and the chance of the transpositions being due to guesses is obviously negligibly small. It is, to say the least, difficult to account for these results on Broadbent's model. If the two inputs are separated before the filter in separate stores then this sort of error could not occur, since the channels are distinct. If the transpositions are supposed to occur after the filter, when the material is all together in the conventional short term memory system, then the number of errors in all is even further reduced to a level now far, far below what would be expected in terms of the time constant given by Broadbent.

In conclusion, it may be said that the present evidence is not strong enough to show that the relevant parts of Broadbent's model are actually wrong; but this is in part at least due to the vagueness of those parts of the model, in particular Postualtes H and J, (Broadbent, 1958, pp. 298, 299). The writer agrees with Broadbent that there is a limit on performance due to switching time, but feels that it has not been measured. He is also a firm admirer of the "Filter Theory" as a general concept. The present paper is more concerned to point out what parts of Mr. Broadbent's lily require gilding than to uproot one of the more attractive blooms in the garden of contemporary psychology.

The writer wishes to thank Professor R. C. Oldfield for providing facilities during the former's time as a research student at the Oxford Institute of Experimental Psychology, during which time half the present work was done. His thanks are also due to the Medical Research Council for a scholarship which enabled him to undertake the work; and to Dr. R. Davis who supervised the research. Lastly he wishes to acknowledge the help and encouragement received from D. E. Broadbent, of the A.P.R.U., Cambridge; and Miss A. Taylor for much stimulating discussion.

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AN EXPERIMENTAL SYNTHESIS OF THE ASSOCIATIONIST AND GESTALT ACCOUNTS OF THE PERCEPTION OF SIZE. PART III

BY

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Two control experiments are described which are concerned with the effect, on judgements of size, of varying the angular separation of the compared objects in the horizontal plane. The first experiment investigates order effects. The second experiment compares judgements of real size at small and large angles of separation. The findings are discussed in relation to parts I and II of these papers.

Introduction

Parts I and II (Joynson, 1958, *a* and *b*) described an experiment which investigated the effects of angular separation and perceptual attitude on judgements of size. The experiment (afterwards referred to as "the original experiment") was exploratory, and many points require further examination. Two control experiments are described here.

Experiment I

In the original experiment, two main types of judgement were found, "real" (R) and "other than real" (N). Most observers made R judgements first, in order of separation from 40° to 0°; and then N judgements, in order from 0° to 40°. Thus a control experiment is required in which the order is reversed. In the present experiment, N judgements are obtained first in order 40° to 0°, followed by R judgements in order 0° to 40°. So data is obtained complementary to that given in Joynson (1958, *b*, Table I).

A point of special importance which arises is that, in the original experiment, many observers did not seem to be aware of the N possibility initially at 40°. There seemed to be great individual differences in readiness to notice it. Most N judgements at 40° in the original experiment were made only after experience at 0°, where the N possibility is more noticeable. It is therefore especially desirable to determine how far we can obtain N judgements at 40° without this prior experience at 0°.

Apparatus and procedure

The apparatus was identical with that used in the original experiment. Twenty observers, with no knowledge of experimental psychology, compared a 5 cm. rod at 100 cm. with a variable rod at 300 cm. Separations of 40°, 10°, 5° and 0° were again used.

To obtain N judgements, the following instructions were given. They are based on the descriptions of N judgements given in the original experiment. "Do *not* try to make the rods the same real size. They are *not* to be adjusted so that, if measured with a ruler, they would be the same physical length. They are to be made *apparently* equal only. You are *not* to allow for the distance. Judge when they just *look* the same size, *not* when they *are* the same." Each made six judgements at each angle, in the order 40° to 0°, the method of adjustment being used. Observers were instructed to use binocular vision only. They were not told that the angle of separation was to be reduced, nor were they given any information about the possibility of using either simultaneous or successive fixation of the rods (Joynson, 1958, *a*).

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When they had completed these judgements, they were told: "Now judge when the rods have the same real size, so that if measured with a ruler they would be the same." Each observer made six judgements at each angle, in the order 0° to 40°. They were also instructed here to make a successive comparison, since R judgements in the original experiments were made in this way. At the conclusion of the experiment, observers were again asked to describe what they had been doing as fully as possible.

Initial responses

The first instructions required the observer to make a judgement other than a judgement of real size. Initially, at 40°, there were considerable individual differences among observers in ability to do this. All seemed to consider that a judgement of real size would be fairly straightforward, but not all could see the possibility of something other than this, which would conform to the instructions. A classification is arbitrary, but observers may be roughly grouped as follows. Seven observers seemed to grasp the instructions rapidly and easily (group A: mean = 6·29 cm.). Six understood them, but more slowly and with greater apparent difficulty (group B: mean = 6·19 cm.). Five said they had great difficulty (group C: mean = 5·32 cm.), and it seems doubtful whether they were distinguishing clearly. In these five cases, the difference actually made between R and N judgements was less than 0·5 cm. The remaining two observers (group D: mean = 4·75 cm.) were quite unable to make anything other than a real judgement.

In the case of groups C and D, observers could see the possibility of judging when the rods looked as if they were really the same (i.e. an R judgement), but they could not clearly see any possibility of a judgement contrasted with this, in which the rods just *looked* the same, but were not really equal (i.e. an N judgement). Every effort was made to obtain an other than real judgement, for example by repeating the instructions and explaining them. But this was ineffective. In the case of group C, it seems reasonable to suppose that the small difference which they actually made between R and N judgements, was made, at least in part, because they felt they were expected to make a distinction. Groups A, B and C (eighteen observers) have, however, been combined in the following report, since all made a clear distinction at lesser angles, and all made at least some approximation to an N judgement at 40°. The judgments of these eighteen observers will be described next. The two observers in group D are considered later.

Of these eighteen observers, sixteen made the successive comparison (NSC judgements) at 40° initially, and only two made the simultaneous comparison (NSM judgements). As angle was reduced, an increasing number spontaneously changed from the successive to the simultaneous comparison. Thus at 10° only fifteen made the successive comparison, at 5° only nine, and at 0° only three. As in the original experiment, only approximations to a strictly simultaneous comparison were made at wider angles. When asked to make R judgements, beginning at 0°, a few observers stated initially that the conditions seemed too hard. They were told that they would probably find it easier than they expected, and in fact, encountered no special difficulty.

Quantitative results

The results are given in Table I, and should be compared with Joynson (1958, *b*, Table I and pp. 144–146). Four measures have been calculated: (1) mean values, (2) variance in mean values, (3) mean variability within observers, and (4) variance in mean variability. As in the original experiment, the last three measures show a strong tendency to vary together within each type of judgement, and again they may be taken in combination as a measure of the "reliability" or "consistency" of the judgements. The data have been subjected to the same statistical analysis, in order to assess differences between the three types of judgement at each angle, and to assess trends with angle within each judgement. In addition, the significance of differences between judgements in this experiment and in the original have been calculated (Table II). In the case of N judgements, differences are assessed at each angle, where numbers are large enough to permit comparison. In the case of R judgements, differences are assessed over the four angles in combination. The general pattern of results in the two experiments is very similar. In view of this,

the full statistical results will not be given. The similarities between the two experiments will be stated first, and then the differences.

TABLE I

QUANTITATIVE RESULTS (EXP. I)

Measure (cm.)	*Judgement*	*Angle of separation*							
		0°	*n*	5°	*n*	10°	*n*	40°	*n*
Mean values	NSM	14·958	15	13·080	9	10·528	3	8·808	2
	NSC	10·534	3	7·537	9	6·921	15	5·683	16
	R	5·648	18	5·392	18	5·240	18	5·118	18
Variance in mean values	NSM	0·168		2·921		1·192		1·109	
	NSC	7·208		0·721		0·944		0·290	
	R	0·953		0·702		0·470		0·368	
Mean variability within observers	NSM	0·174		0·521		0·573		1·037	
	NSC	0·944		0·446		0·346		0·297	
	R	0·374		0·288		0·250		0·252	
Variance in mean variability	NSM	0·0134		0·0695		0·0180		0·3890	
	NSC	0·1210		0·0351		0·0361		0·0233	
	R	0·0180		0·0177		0·0095		0·0088	

(1) *Similarities*

(i) NSM judgements. At 0°, these are not significantly different from 15 cm. ($t = 0{\cdot}40$; $p > 0{\cdot}05$), the correct figure for relative retinal length, and here they show a good level of reliability. They are again strongly affected by angle, and as separation increases they fall below 15 cm. and show a poor level of reliability.

(ii) NSC judgements. These fall between 5 and 15 cm. at 0°, where they again show poor reliability. They are strongly affected by angle, like NSM judgements, though in the opposite way with respect to reliability. As separation increases, they approximate to R judgements in both mean value and reliability, though at 40° they are still significantly greater in mean value than R judgements ($t = 2{\cdot}86$; $p < 0{\cdot}01$).

(iii) R judgements. At 0°, they are significantly greater than 5 cm., the correct figure for equal physical length ($t = 2{\cdot}82$; $p < 0{\cdot}05$), but they are not significantly different from 5 cm. at 5°, 10° and 40° ($t = 1{\cdot}99$, $1{\cdot}49$ and $0{\cdot}83$ respectively, $p > 0{\cdot}05$). They show a good level of reliability throughout, except that variances in mean values are rather greater than in the original experiment. They are comparatively little affected by angle, but, in so far as they are affected, are similar to NSC rather than NSM judgements.

(2) *Differences*

(i) There are marked differences between the two experiments in the proportions of observers making NSM or NSC judgements at wider angles. At 40° in the original experiment eleven of twenty-four made the NSM comparison, as compared with only two of eighteen in this experiment. At 0°, on the other hand, the distribution is very similar: in the original experiment twenty-two of twenty-four made the NSM comparison, compared with fifteen of eighteen in this experiment.

(ii) There is a significant increase in this experiment, in three out of four cases, in variances for NSM judgements at 0° and 5° (Table II).

(iii) There is a significant increase in this experiment for R judgements in both mean value and variance in mean value (Table II).

TABLE II

SIGNIFICANCE OF DIFFERENCES BETWEEN MEAN VALUES IN EXPERIMENT I AND IN THE ORIGINAL EXPERIMENT

Measure (cm.)	*Judgement*	*Angle of separation*			
		0°	5°	10°	40°
Mean values (t)	NSM	0·17 N.S.	1·44 N.S.	—	—
	NSC	—	1·17 N.S.	1·31 N.S.	1·34 N.S.
	R	Overall : 2·71*			
Variance in mean values (F)	NSM	1·20 N.S.	3·01*	—	—
	NSC	—	1·18 N.S.	1·24 N.S.	1·79 N.S.
	R	Overall : 2·38*			
Mean variability within observers (t)	NSM	1·15 N.S.	0·47 N.S.	—	—
	NSC	—	0·65 N.S.	0·24 N.S.	0·66 N.S.
	R	Overall: 0·29 N.S.			
Variance in mean variability (F)	NSM	8·93†	3·51*	—	—
	NSC	—	2·58 N.S.	1·02 N.S.	1·49 N.S.
	R	Overall: 1·22 N.S.			

N.S. = $p > 0.05$. * = $p < 0.05$. † = $p < 0.01$. t_{FB} is used where F is significant.

(3) *Practice effects*

Practice effects for both experiments were assessed by comparing the mean of the observer's first two judgements with the mean of his last two. There was some evidence of moderate trends in the following cases. (i) NSM judgements at 5°: mean practice effect for both experiments combined = + 0·52 cm., i.e. in the direction of reduced constancy (fourteen of twenty observers show the trend). (ii) NSM judgements at 40°: mean practice effect for both experiments combined = + 0·36 cm. (ten of thirteen observers show the trend). (iii) NSC judgements at 0°: mean practice effect for both experiments combined = + 0·30 cm. (ten of fifteen observers show the trend). (iv) R judgements at 0° in the present experiment only (this forms a further difference between the two experiments): mean practice effect = — 0·28 cm., i.e. in the direction of greater constancy (fourteen of eighteen observers show the trend).

It will be seen that practice effects tend to appear where the "reliability" of the judgements is comparatively poor, as might be expected.

Qualitative results

Special emphasis is again placed on observers' reports, and the following should be compared with Joynson (1958, *b*, pp. 146–151).

(i) *N judgements*

There were many comments from observers at 40° initially concerning the difficulty of obeying the instructions and avoiding a judgement of real size. E.g. O.2: "I find it almost impossible to ignore the difference of distance"; O.8: "This is hard work you know.

I feel I have got to fight against taking distance into account"; O.10: "It is very difficult. I have to keep reminding myself not to judge actual size"; O.15: "It is difficult when you can see that the distance is different. I must just try to struggle against it." These comments were not restricted to observers in groups C and D who, as we saw, had particular difficulty in avoiding R judgements here.

As angle decreased, several observers seemed to become increasingly aware of the strength of this tendency. E.g. O.5 (at 10°): "You know, I don't think I'm doing this right. I think I might be half unconsciously doing what you said I was not to do." O.8 (at 10°): "I am just struck by a terrible doubt that I have not been disregarding distance." Finally at 0° they seemed to consider that they were now at last making the judgement with some accuracy, and that earlier judgements were inaccurate, in that they had been unconsciously making much more allowance than they had supposed. E.g. O.2: "I rather suspect that all along I have been making some unconscious allowances for distance, though I was trying not to"; O.8: "I have come to the conclusion my previous judgements were all to hell. I have been progressively more and more conscious of the need to reject the habitual reaction of allowing for distance"; O.11: "I don't know what I was up to at first now"; O.12: "I think, I tended to do that (i.e. allow for the distance) more than I thought at the time." There were also several expressions of surprise at 0°, which again seem to indicate that observers had been allowing for the distance more than they supposed. At 0°, observers again state, as in the original experiment, that the NSM judgement is concerned with "sheer eyesight" or "mere appearance." Here observers are aligning the tops and bottoms of the three images obtained when one rod is continuously fixated.

The two anomalous observers (group D) may be considered here. Both gave judgements approximating to 5 cm. at 40°, 10°, and 5°, and seemed quite unable to make N judgements. At 0°, one suddenly saw the possibility: "Oh! I see I have been compensating for distance. Good god. Fantastic, isn't it? Good lord, amazing." He was, however, still unable to distinguish at wider angles. The second simply made a simultaneous judgement at 0°, approximating to 15 cm., and considered this also was an R judgement. He did not succeed in making a successive comparison at 0°, nor in drawing the distinction between R and N judgements at any angle.

Group C, who had difficulty in distinguishing initially, were asked to repeat N judgements at 40° after experience at 0°. They then gave a mean of 6·82 cm. and now seemed able to distinguish clearly, except for one observer. It will be seen that seven observers of the total twenty (groups C and D: 35 per cent.) could not clearly distinguish the N possibility at 40° prior to experience at 0°, and three observers (15 per cent.) still could not clearly distinguish even after experience at 0°.

(2) *R judgements*

At the conclusion of the experiment, observers were asked to describe the R judgement as accurately as possible. Their descriptions are similar to those obtained earlier. We cannot place emphasis on reports of "allowing for the distance," since this had been mentioned in the instructions. But most observers seemed to find this an appropriate phrase, and there were many comments that the R judgement involves something more than "sheer eyesight," as compared with the N judgement. E.g. O.1: "The apparent judgement doesn't make use of eyesight so much" (i.e. the R judgement makes more use of eyesight).

Most observers again found it difficult to elucidate the nature of this activity, and were intrigued and tantalized by the attempt to do so. O.7: "It is very difficult to say. Language rather fails to describe processes like that"; O.12: "It is difficult to say . . . it is rather queer." Most observers then made comments which suggest that the activity can roughly be called "intuitive looking." O.2: "It is purely instinctive. The distance is taken into account automatically"; O.6: "There just comes a point where I've got to say stop" O.9: "It is just a feeling of rightness." This process involves how the rods "look." O.3: "It is a visual impression more than anything"; O.5: "They just looked right . . . intuition"; O.10: "It is simply when it looked about right." But this involves something more than the "mere appearance" of the N judgement: O.12: "They do not *look* to be identical objects"; O.13: "Something more is involved than just *looking*, but it is subconscious. It is like trying to say how you talk." As this last quotation indicates, the judgement does not seem to involve conscious calculation. As before, comments seem to suggest that the judgement is neither "sheer eyesight," nor a process of conscious calculation, but something in between.

A few observers were again able to go rather further than this, and described the process which was termed "potential looking": that is, they judge when the far rod has that "mere appearance" which they suppose the near rod would have at that distance. O.4: "You look at one, and carrying it in your mind's eye to where the other is, imagine how big it would be there, and then make the other equal to that"; O.16: "I ask myself what would something 'that big' look like at that distance"; O.18: "I have an instinctive idea of how an object the same actual size as the nearer one would appear at a certain distance." Five of twenty observers made such reports, as compared with ten of thirty in the original experiment. Their comments again suggest that in every day life the process would be carried out automatically, and only in the experimental situation do they become reflectively aware of it.

A few observers again mentioned the role of the surroundings. O.15: "In judging the apparent, you forget all about the surroundings, and just look at them as pieces of black. You try to shut everything else out. In the real, you take account of the difference of distance. You allow everything else to influence your judgement. It is much easier." Lastly, in this experiment too, most observers (fifteen of twenty) considered the R judgement to be the more normal and everyday, and again some were most emphatic about this: e.g. O.15: "Very definitely. One doesn't live in two dimensions"; O.16: "Obviously. Nobody every worries about apparent size."

Discussion

Quantitative findings

The quantitative value of *N judgements* is broadly similar, whether they are made in the order 40° to 0°, or the reverse. It might have been expected that N judgements at wider angles would be markedly higher if observers have had prior experience at 0°, than if this experience is absent. But the experiments suggest that any such effect is likely to be of a comparatively minor order. It seems that N judgements deviate from 15 cm. at wider angles because observers cannot help making some automatic allowance for distance. They are able to inhibit this completely if they make the NSM comparison at 0°. But the failure of such experience at 0° to exert a more marked effect at wider angles, emphasizes the automatic character of the allowance. We saw that some of these judgements were tending to lesser constancy with practice. Whether more prolonged practice would be more effective, the experiment does not enable us to state.

Differences between the two experiments do, however, arise in the proportions of observers who make the two types of N judgement. We have seen that many more observers choose to make the NSM judgement at wider angles in the original experiment. The explanation is presumably that, in the original experiment, observers began at 0° by making the NSM comparison, and attempted to continue this as separation increased, feeling it to give more accurate judgements. But in the present experiment, they begin at 40° making the NSC comparison, because the NSM comparison is difficult to notice. It becomes more easily noticed as separation decreases, and observers then change to it. This is again because they feel it to be more accurate, and so at 0° the proportions making the NSM comparison are similar. Thus the effect of different orders of presentation lies primarily in the proportions choosing to make the different kinds of judgement. Its effect on the quantitative value of those comparisons, once they have been made, seems to be of secondary importance. The explanation of the significantly greater variances for NSM judgements at 0° and 5° in the present experiment, is presumably connected with this. It is probably because many observers were here changing from the NSC to the NSM comparison, with a consequent disturbance of their judgements.

The significant increase for *R judgements* in mean value and variance in mean value, as compared with the original experiment, requires explanation. This might be because, in this experiment, R judgements are made immediately after N judgements at 0°. Since observers have been adjusting the variable towards 15 cm. in the

N judgement, they may tend, when making the R judgement, to adjust the variable to a rather greater length than would otherwise seem appropriate. Alternatively, the increase may be a consequence of making R judgements in the order 0° to 40°, rather than in the reverse order.

To decide between these two interpretations, a *supplementary experiment* was conducted with ten additional observers. Using the same apparatus, each made six R judgements at 0° without previous experience of any kind. They gave a mean of 5·20 cm., with variance 0·417 cm. Since this mean is slightly *below* that in the original experiment, and the variance is not significantly different, the first interpretation above is favoured: the greater values in Experiment I are more likely to be a consequence of the immediately preceding N judgements than of making R judgements in the order 0° to 40°. The practice effect in Experiment I for R judgements at 0° is possibly also a function of the preceding experience, since in the supplementary experiment there was no evidence for a practice trend. The further question of why, in both experiments, R judgements are somewhat impaired at 0°, as compared with 40°, is considered in the next experiment.

Qualitative findings

In general, the results substantiate those of the original experiment. The finding that seven observers of the total twenty (35 per cent.) could not clearly distinguish the N possibility at 40° initially, is in good agreement with the conclusion in the original experiment that at least twelve of thirty (40 per cent.) were not aware of it at 40° initially. It seems that, under the circumstances of this experiment, some third of observers find it very difficult, if not impossible, to adopt a so-called "analytic attitude" at wider angles. The reports that, as separation decreased, observers increasingly realized how much they had unconsciously allowed for the distance, and that N judgements are more accurate at 0°, are complementary to those of the original experiment, in which it was said to become increasingly difficult to avoid allowing for the distance as separation increased, and that N judgements were less accurate there.

Experience at 0° seems to facilitate the noticing of the N possibility at 40° in some cases, since of the seven observers just mentioned, four were able to make the N judgement at 40° after experience at 0°. But three (15 per cent.) were still unable to distinguish it. This last finding suggests that in the original experiment, where observers made N judgements at 40° after experience at 0°, a small proportion could probably not in fact distinguish these clearly from R judgements. This suggestion is supported by the observers' comments there, namely, that NSC judgements were not very different from the original R judgements. On re-examining the quantitative results, it has been found that, of the ten observers making the NSC comparison at 40°, three (10 per cent.) differ by less than 0·5 cm. from their R judgement. This is in good agreement with the present experiment.

The observers' descriptions of R judgements confirm those obtained in the original experiment. One earlier conclusion is especially emphasized, namely, that at wider angles observers are less likely to be aware of the activity involved in the R judgement. Seven observers were unable to distingish initially between "when they *are* the same" and "when they just *look* the same." Only as separation was reduced, did they become aware of the activity. This again raises the question whether R judgements differ at different separations.

Experiment II

This experiment is concerned with the comparison of R judgements at 0° and 40°. In both experiments, R judgements are significantly greater than 5 cm. at 0°, and show

greater mean values at 0° than at 40°. They also show poorer "reliability" at 0°, though the trend with angle is significant only in the case of mean variability. This poorer performance in R judgements at 0° is accompanied by several reports of greater difficulty there than at wider angles.

This experiment is concerned with the question whether these are invariable characteristics of R judgements at 0°. If they are, it could be argued (see below) that the situation at 0° is artificial and that the R judgements and reports obtained there are not representative of normal perceptual judgements. It is also possible, however, that R judgements are impaired at 0° because cues to the distance of the farther rod are inferior to those at 40°. At 0°, in both experiments, the stand upon which the farther rod was mounted was almost completely hidden from the observer behind the nearer stand. This might well impair perception of the distance of the farther rod, and reduce constancy. In this experiment, cues to distance are equally good at 0° and 40°, so that we can determine whether in this case R judgements are impaired at 0°. In addition, each observer made thirty judgements, in order to determine whether practice effects occur to any marked extent.

Apparatus

This was the same as in the earlier experiments, except that the far rod now appeared immediately in front of a white cardboard screen, 80 cm. sq. This provided the observer with an excellent indication at 0° of the plane in which the far rod was situated, since it was easily visible beyond the nearer stand. The standard rod was again 5 cm. at 100 cm. distance, and was 0·07 cm. in diameter. The variable was again at 300 cm. and was 0·2 cm. in diameter. The bases of the rods again appeared to the observer in the same horizontal plane.

Procedure

Forty naïve subjects were divided randomly into two groups of twenty. One group made judgements at 0° only; the other group at 40° only. The instructions were as follows: "Judge when the rods are the same real size; that is, state when you think the rods are the same number of inches in length. Please make your judgements carefully, but do not try to calculate in any way. Just rely on your judgement. You will probably find it easier than you might imagine." Observers were also instructed to make a successive comparison of the rods, and to use binocular vision. Each made 30 judgements. The method of adjustment was used, the initial position of the variable being randomly varied between 0·5 cm. and 14·5 cm. No practice judgements were given.

Results

(1) The results are given in Table III. There are small differences between 0° and 40°, in the same direction as in the earlier experiments, but none of these differences is significant.

TABLE III

QUANTITATIVE RESULTS(EXP. II)

Measure (cm.)	*Angle of separation*		*Significance*
	0°	40°	
Mean values	5·028	4·905	$t = 1{\cdot}09$. $p > 0{\cdot}1$
Variance in mean values	0·126	0·116	$F = 1{\cdot}09$. $p > 0{\cdot}05$
Mean variability	0·358	0·331	$t = 0{\cdot}59$. $p > 0{\cdot}05$
Variance in mean variability ..	0·0264	0·0154	$F = 1{\cdot}71$. $p > 0{\cdot}05$

(2) Since the means are not signicantly different, and since they lie on either side of 5 cm., neither is significantly different from 5 cm., the correct figure for equal physical length.

(3) To assess practice effects, a comparison was first made between the mean of each observer's first and second judgements and the mean of his fifth and sixth judgements. There is no evidence of any important trend. At 0° observers are equally divided in direction of trend shown. The mean trend is — 0·075 cm. (i.e. in the direction of greater constancy). At 40°, ten show a trend in the direction of greater constancy, eight show the reverse trend, and two show no trend. The mean trend is — 0·055 cm.

(4) To assess practice effects over a greater number of judgements, each individual's judgements were next divided into first ten, second ten, and third ten. At 0° the successive means were 5·12, 4·97 and 4·98 cm., and at 40° 4·98, 4·90 and 4·83 cm. Analysis of variance (Table IV) shows a significant overall practice effect at 40° ($F = 5{\cdot}40$; $p < 0{\cdot}01$), but not at 0° ($F = 3{\cdot}07$; $p > 0{\cdot}05$). The analysis also shows (*a*) that there is a significant practice effect for some individuals at 40° ($F = 1{\cdot}82$; $p < 0{\cdot}05$), and at 0° ($F = 3{\cdot}29$; $p < 0{\cdot}001$); and (*b*) that there are significant individual differences at 40° ($F = 18{\cdot}0$; $p < 0{\cdot}001$), and at 0° ($F = 8{\cdot}52$; $p < 0{\cdot}001$).

TABLE IV

ANALYSIS OF VARIANCE (EXP. II)

Source	*d.f.*	0°		40°	
		S.S.	*Variance estimation*	*S.S.*	*Variance estimation*
Individuals	19	74·49	3·92	68·47	3·60
Practice	2	2·82	1·41	2·16	1·08
Interaction	38	17·60	0·46	7·73	0·20
Within individuals	540	75·20	0·14	60·91	0·11
Totals	599	170·11		139·27	

(5) Observers' reports did not suggest that there were any important differences between judgements at the two angles.

DISCUSSION

The main finding of the experiment is that, when distance cues are equally good at 0° and 40°, there is little if any difference between R judgements at the two angles. Thus the impairment of R judgements at 0° found in earlier experiments may be ascribed to poorer distance cues there, and is not a necessary characteristic of small angles of separation. There is also no evidence that practice effects are more likely to be found at 0° than at 40°.

Emphasis has been placed on this experiment for the following reasons. We have argued that the R judgements obtained in these experiments correspond closely to our everyday perceptual impressions, and involve an active process. The observers' reports, about "allowing for the distance" and knowing how "mere appearance" varies with distance, have been cited as part of the evidence for this. We have also suggested that the activity is usually carried out automatically, but that observers are more likely to become aware of it at small degrees of separation, where the N possibility is more noticeable (Joynson, 1958, *b*). These conclusions are in contrast with those of the Gestalt psychologists. They hold that our everyday perpetual

impressions involve nothing but a passive "direct experience." They might presumably argue that the observers' reports concerning an activity are primarily derived from 0°; that R judgements here are different in nature from those at 40°; and that the situation at 0° is an artificial one, which throws little light on normal perceptual judgements. Thus Koffka (1935) advocates that the comparison objects should be placed in different directions from the observer, and writes that "if the two objects are too close to each other in the visual field they will influence each other and so distort the picture of uninfluenced constancy." Presumably on this view an impairment at 0° is always to be expected.

It might have been claimed that there was some evidence for this "influence" and "distortion" in the results of the original experiment and Experiment I. But it now seems that the impairment at 0° was a function of our particular experimental set-up, and is not an inevitable occurrence. The results strengthen our conclusion that there is no fundamental difference between R judgements at the two angles, and accordingly the value of the descriptions of R judgements which we obtained does not seem to be impugned. As stated earlier, observers' comments "indicate that they become more aware, at smaller angles, of what they have been doing all the time, not that they change from doing one thing to doing another" (Joynson, 1958, *b*). What happens at 0° is not that the constancy of the everyday perceptual judgement breaks down, but that the possibility of making a different kind of judgement becomes much more noticeable, and the active nature of the everyday judgement becomes evident.

Conclusion

These experiments provide support for the earlier conclusions, and so help to relate the contrasting Associationist and Gestalt accounts of the perception of size. We must in conclusion emphasize a central limitation of the study. The main point of dispute between the rival theories concerns the nature of our everyday perceptual impressions. In these experiments, observers have asserted that R judgements correspond closely to such everyday impressions. But they have also frequently stressed that the experimental judgements are made more deliberately and reflectively than would normally be the case. So caution is necessary in generalizing from the results. It would be desirable to try to obtain less deliberate judgements, in an attempt to approximate more closely to the everyday impression. In conjunction with this, it would be desirable to try to elucidate the conception of real size.

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SHORTER ARTICLES AND NOTES

APPARENT MOVEMENT AND THE BROWN-VOTH EXPERIMENT

BY

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An experiment on apparent movement reported by Brown and Voth and referred to by Osgood and Bartley was repeated. The results obtained differed from those reported by Brown and Voth, but exhibited phenomena not described by them. Further experiments derived from their theories were also carried out, the results failing to support Korte's laws and raising some new points.

Introduction

In his *Method and Theory in Experimental Psychology* (1953) Osgood gives considerable regard and space to a paper by Brown and Voth (1937) on apparent movement, in which an experiment is described where four lights disposed at the corners of a square are flashed on and off successively, going round the square. Bartley (1941) also devotes some pages to this. According to these authors, when the interval between successive lights in such a set-up is around 150 millisec., a single light is seen to describe a circle. They furthermore claim that this circle has a circumference falling well within the actual square of the flashing lights, as a result of the "cohesive forces" of Gestalt theory. As the interval between lights is further shortened, they say, the circle enlarges again until (at 50 millisec.) four lights are seen simultaneously.

Osgood describes this experiment as "Certainly the most compelling application of this general Gestalt theory of perception" and later says "Careful measurements fully substantiated these predictions from theory as well as many others . . . ." Bartley says much the same. It may be of interest, therefore, to report a repetition of the experiment in this department which has (*a*) failed to obtain the results claimed by Brown and Voth and (*b*) obtained quite different results, namely the occurrence of straight-line Beta movement (optimal movement) in a variety of patterns which accord neither with field theory nor with Korte's laws. Two subjects only were used in the Brown-Voth experiment: fifty-eight subjects were used in the experiments to be described.

Experimentation with the Brown-Voth apparatus was begun with the idea of *using* the Brown-Voth phenomenon (in connection with an experiment on drug effects) and the procedure at first employed differed slightly from theirs; but this, according to their theory, should have made little or no difference to the appearance of the phenomenon. Since, however, the results actually obtained in preliminary trials with the apparatus were quite unexpected, it was decided to reduplicate exactly the Brown-Voth conditions. This introduced difficulties. In the first place, while they give details of light-size and the distances between lights, they do not give the viewing distance (the visual angle, hence, cannot be obtained). There is a further difficulty: they state that "The light boxes A, B, C, D, were duplicated on adjustable grooved arms and with adjustable apertures to allow objective comparison

of the displacements of movement paths with a static field. The light boxes for comparison will be called Ac, Bc, Cc, Dc. The general experimental procedure was to allow S to adjust Ac, Bc and Dc (this was taken to mean that in fact only three comparison lights were used) to phenomenal identity with the certain points of the path of movement as perceived in the field of the main apparatus." Now, if the subject is to reach out and adjust a comparison apparatus adjacent to the display, he cannot be much more than 2 ft. away from it. When the display is a square of side 2 ft. (Brown-Voth experiment III), which give a visual angle of over 53°, the display itself is entirely peripheral (without large eye-movements) and to take in a comparison apparatus as well would be impossible. Large eye movements, of course, destroy the apparent movement under investigation. In view of these considerations, in the experiment to be described the subject was seated 6 or 8 ft. from the display and the comparison apparatus, when used, was adjusted by the experimenter.

A final point: Brown and Voth talk of a "square" but do not say at what angle to the horizontal this square is presented. Their diagrams are drawn with the diagonals of the square horizontal and vertical but their apparatus shows that the angle could be readily adjusted. Bartley (1941) evidently interpreted the presentation as with the square upright. In any case, of course, the plane angle of the square should not affect the results in any way, according to the theory. In the first two experiments described in this paper the presentation was "diamond-form," in the remainder the square was upright.

The first experiment described was the attempt to replicate exactly the Brown-Voth experiment. The others were in fact carried out first.

Method

Experiment 1. Four circular light-boxes (made from 2 in. diameter pill-boxes) were arranged at the corners of a square A B C D of side 6 in. and presented in the form of a "diamond," i.e. with one diagonal vertical. The light-boxes were fitted with 6·3 volt bulbs which could be flashed successively (A, B, C, D, A, etc.) by means of a rotating eccentric arm actuating four contacts equidistantly spaced. The front of each light-box was covered by a lid which had cut in it a circular aperture $\frac{1}{4}$ in. in diameter. Duration of the lights was controlled by varying the eccentricity of the rotating arm: time interval between lights was varied by adjusting the speed of the motor driving the arm and this motor was geared to a small D.C. permanent-magnet generator which activated a calibrated galvanometer and so continuously indicated the time-interval between successive light stimuli. The timing mechanism was situated in a room other than the experimental one, to prevent the use of auditory cues, and the experiment was carried out in the dark, the experimenter's light being adequately screened. The lights were presented flashing in rotation clockwise with intervals between successive lights (*ti*) of 300 millisec., 250 millisec., 200 millisec., 150 millisec., 100 millisec. and 50 millisec. and the duration of each light (*tl*) was equal to the interval between lights, as in the Brown-Voth experiment. Three subjects were used, a physician, a physicist and an actress. They were seated 8 ft. from the display, and each was given eight trials at each frequency in random order, four with the comparison lights starting on a square greater than 6 in., four with the comparison square starting less than 6 in., alternatively. The experimenter adjusted the comparison lights according to verbal instructions.

Experiments 2–5. Details of these are given with the appropriate experiment in the Results section.

Results

Experiment 1. Table I compares the data given in Table I of Brown and Voth's paper with the data obtained in the present experiment. It will be seen that optimal circularity with a diminution of size was not obtained. What was reported, however, at 100 millisec., was that two lights were moving to and fro along the straight sides of the square, or else that a light was moving up and down the diagonals—optimal, straight-line Beta-movement.

Experiment 2. The comparison apparatus was not used, the experimenter merely saying: "In a moment I shall switch out the room light. I want you to keep looking straight ahead and tell me what you can see—keep up a continuous running commentary, so to speak. Do not try and guess what you *ought* to see but simply describe how things appear to you." The apparatus was then switched on and the speed of the motor operating the contacts slowly increased until well past the subject's report of simultaneity and then slowly decreased again until his report of succession. Five post-graduate psychology students were used as subjects.

TABLE I

SUBJECTS, REPORTS AND ESTIMATIONS OF SIZE OF SQUARE (IN INCHES)

	Time interval between successive lights (in millisec.)					
	300	250	200	150	100	50
Mean .. .. A	6·28	5·78	5·74	5·41	5·93	5·95
Prevalent type of movement	Partial on side of square	Optimal on side of square	Optimal with some circularity	Optimal with optimal circularity	Partial simultaneity confused	Simultaneity
Mean .. B	6·0	6·0	6·0	5·9	6·1	6·0
Prevalent type of movement	Succession	Succession	Beta movement jerking round square	Beta round square and D-shaped Beta movement	Beta up and down vertical diagonal, to and fro along horizontal diagonal and to and fro opposite sides of square	Simultaneity

A = Brown-Voth Data. B = Data obtained in this experiment.

Subjects reported seeing, first, four lights appearing and disappearing one after the other successively round a square. Then, as the interval between lights (*ti*) was shortened, they reported that it seemed as though one light was tracing out—rather jerkily—a square. This became smoother as the interval diminished then suddenly two lights in linear Beta-movement were seen, along opposite sides or along the diagonals of the square. Average intervals for the five subjects were (*a*) for one light moving round square (threshold to succession) $ti = 129$ millisec. (*b*) two lights Beta-movement: $ti = 93{\cdot}4$ millisec. (*c*) simultaneity: $ti = 76{\cdot}0$ millisec. Two of the subjects also reported that above the threshold for simultaneity they saw "a sort of whirlpool in a black cloud" (subject 1), "something black swinging round in front of the lights" (subject 2).

Experiment 3. The method was as in experiment 2, except that (*a*) the relative duration of the lights was shortened so that it was half the time interval between two successive lights: (*b*) the light aperture was increased to ½ in.: (*c*) subjects were seated 6 ft. from the display: (*d*) lights were presented at the corners of an upright square. After one or two "up" and "down" runs, the time interval (*ti*) was moved slowly between 20 and 40 millisec., the subjects' reports of the "ghost" being noted. Eight engineering apprentices were used as subjects.

At mean *ti* of 92–64 millisec. (range 113–52 millisec.), all subjects reported Beta-movement, either two lights moving up and down the two vertical sides of the square, or two lights moving to and fro along the horizontal sides of the square, or a light moving alone one or both diagonals.

From time to time the patterning would suddenly change from one form to another in a manner similar to the alternations seen with the Necker cube. Each light appear to move to and fro or "bounce" up and down regularly and smoothly.

At longer intervals, four subjects reported circularity but it was not nearly such optimal movement as the straight-line movement and would be better describable as *phi*. No subject reported a diminution of the circle, though all were specifically asked. Six subjects reported the "ghost"—the black swirling rotation. Some of them said that it appeared not so much "black" as "negative"—a nothingness revolving round the midpoint of the circle.

Experiment 4. The square was increased to a side of 7½ in. and a fixation point was added, in the form of a white light 1 m.m. in diameter at the centre of the square. Also, as it was thought probable that the rotatory after-image phenomenon was to some extent affected by the die-away time of the filament bulbs, four 90 volt neons were substituted for them in this experiment. Ten subjects were used, comprising lecturers, research workers and students in psychology and physiology. The "ghost" phenomenon remained, eight out of the ten subjects reporting it. It seemed to take about two minutes' viewing before its onset. At a *ti* of 40 millisec. and less, the lights appeared quite stationary, not flickering but merely obscured by the passage of a dark-green tinted "sweep" or "one-bladed propeller-like thing" rotating in front of them and sometimes blacking out the fixation light. By reversing the direction of rotation of the lights it was shown that the after-image "ghost" would give adequate cue to the direction of rotation at speeds where the lights themselves had fused.

From 150–250 millisec. of time-interval, subjects reported movement of one light going round a square. At 110–150 millisec. five of them reported one light travelling in a circle but their certainty about this was much less than about the 2-light straight-line Beta-movement seen at shorter intervals. One subject reported that the circle became smaller at *ti* = 140 millisec. This subject saw the circle diminish in size on each occasion of several repetitions but when the comparison apparatus was switched on the effect did not occur—and he saw movement in D-shape, the flat back of the D towards the comparison lights, instead of circularity.

With an interval of 60–110 millisec., straight-line Beta-movement of two lights was seen by nine subjects. All of them experienced the Necker-cube-like alternations between horizontal, vertical and diagonal movement. There seemed to be a tendency for the horizontal movement to occur more readily at intervals nearer to the simultaneity threshold (60 millisec.). Five subjects said, when seeing two lights moving up and down the vertical sides of the square, that the left-hand light seemed to be moving faster than the one on the right, and two of these subjects claimed that this difference in speed was confirmed by the fact that the two lights slowly changed phase, sometimes bouncing up and down together, sometimes alternately. These

two subjects also reported that the right-hand light seemed to be travelling further than the left-hand one.

Experiment 5. The set-up was as in experiment 4 with square of side 6 in., viewing distance 8 ft. and light aperture of 2 in. (i.e. the light-box lids were removed). After one increase-and-decrease, the motor speed was set so that the interval between lights was 100 millisec. and the subject was required to state whenever the phenomenal pattern of the display changed (e.g. when it altered from lights moving up and down to lights moving to and fro). The number of these alternations in a period of 2 minutes was recorded. Subjects were 32 engineering apprentices. Five of these subjects reported a circle getting smaller at an interval time of 100–150 millisec. Two saw no apparent movement of any kind. The mean number of alternations in the two minute period was 13·5. Eleven subjects said that the left-hand light moved faster and /or that the right-hand light moved further. The number of alternations correlated significantly ($r = 0{\cdot}438$) with the E-scale of the Maudsley Personality Inventory, but did not correlate with spiral after-effect persistence time (Holland and Beech, 1958). Twenty-six subjects reported the rotating after-image.

Discussion

There is a footnote to the Brown and Voth paper which says "There were some slight differences between the individual observers in the phenomenal descriptions of the path at the various time-intervals. There also seem to be some day-to-day fluctuations in these. The point to be made is that *it is possible* under certain conditions to *create* the predicted sequences." [My italics.] This point was supported by the experiments described here, since six subjects reported a diminishing circle at around 150 millisec. (though not if the comparison lights were on). It is, however, the only point in their whole paper that these experiments did support. The straight-line two-light optimal movement which nearly all subjects saw seems to be a convincing refutation of their theory of "vector-field-forces." With regard to the seeing of a smaller circle it might be pointed out that the experiments were carried out in the dark, making size-distance estimation difficult except when the comparison apparatus and its adjustment by the experimenter furnished visual and auditory cues. The autokinetic effect certainly operated, many subjects reporting a shift of the whole display in one or other direction.

The results obtained do not accord with Korte's laws, either. Let the square on which the lights are dispersed be called A B C D, and consider it when upright and when two lights are seen moving up and down the vertical sides, the lights actually being flashed successively in a clockwise direction. That the appearance of light C 60–100 millisec. after light B should produce apparent movement from B to C is only to be expected; but light B does not again appear until after an interval five times as long as the B–C interval, when $ti = tl$, or four times as long when $ti = 2\ tl$. Why, therefore, should there be movement from C to B apparently at the same speed as, and quite regular with, the B to C movement? (The movement is strikingly clear and looks like a ball bouncing regularly up and down.) Further, it is light D which comes on after C, yet instead of movement from C to D, at intervals shorter than about 130 millisec. what is seen is movement from A to D. The seen movement from C to B, A to D, etc. is evidently illusory in a different way from normal apparent movement.

The writer is unable to see how these facts can be explained by any sort of cortical field-theory or by satiation-theory (Köhler 1940), although this latter theory is widely held as an explanation of apparent movement. At least as far as the $B \rightarrow A$ and $D \rightarrow C$ or $C \rightarrow B$ and $A \rightarrow D$ movement is concerned, the illusion would seem

to be a mistake in interpretation and hence primarily a function of association-pathways and previous experience rather than of the physiology of the retina or the visual cortex. This type of explanation of apparent movement in general is urged by De Silva (1926), Higginson (1926), Pieron (1952) and Hansel (1953), among others, and is reviewed by Toch and Ittelson (1956).

With regard to the reports of the left-hand light moving faster: there would seem to be a connection here with an aspect of the Kappa phenomenon reported by Cohen, Hansel and Sylvester (1955). Explaining the Kappa effect in terms of previous experience of moving objects they predicted and found that when the light was (apparently) moving downwards it did so as though, to the observer, it was accelerating under gravity.

A similar phenomenon to the rotating negative after-image "ghost" has been reported by Saucer (1954) and called by him "Omega movement." He appears to be unaware of after-image phenomena and, so far as I can make out, puts forward an explanation, instead, which says that movement can be perceived when no object is seen to move! Presumably the reason why the lights appear stationary while the "ghost" is seen to revolve is that during the duration of any light the stimulus from it is accompanied by fused after-images of the other three.

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DISCRIMINATION OF SMALL SHAPES BY THE RAT

BY

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Three experiments on small shape discrimination are described. The first investigates the discrimination of square from circle, a discrimination claimed by Lashley to be extremely difficult for the rat and which has since become a matter of theoretical interest. The second and third test a prediction from the writer's theory of shape recognition; the prediction is refuted.

Introduction

It has been pointed out in a previous paper (Dodwell, 1957) that the interpretation of studies of visual shape recognition in rats (e.g. Fields, 1932; Lashley, 1938) is complicated by part-shape discrimination and by "boundary effects." That is to say, rats tend to discriminate in terms of segments of stimulus shapes, rather than whole shapes, and to discriminate by the relationship of a part of the stimulus shape to the edge of the card on which it is displayed. This may invalidate the use of such studies as confirmatory evidence for theories of shape recognition (Deutsch, 1955; Dodwell, 1957, 1958) although Deutsch and Dodwell both claim that Lashley's results lend support to their (conflicting) theories. An attempt was made to eliminate, or at least reduce, boundary effects in an experiment of the writer's (Dodwell, 1957) and the desirability of experimenting with small shape discriminations was pointed out. The experiments here reported are a first attempt in this direction.

Apparatus and Method

The apparatus used was a Lashley-type jumping stand, modified as previously described (Dodwell, 1957). The subjects were male hooded rats from the colony maintained at the Institute of Experimental Psychology at Oxford, all approximately three months old at the beginning of an experiment. They were put on a feeding schedule one week before training started, being fed for two hours every day, and run after approximately twenty hours without access to food in the first experiment. A slightly different schedule was used for Experiments II and III (see below). Method of handling, recording and scoring were as previously described (Dodwell, 1957). The stimulus shapes used were squares, circles and equilateral triangles, of three different sizes, namely squares of side $\frac{3}{4}$ in., 1 in. and $1\frac{1}{4}$ in., and circles and triangles with areas equal to those of the three squares. The shapes were outlined in white ink on matt black cardboard, and the rectlinear shapes were always displayed with their lowest side horizontal, unless a different orientation is mentioned. The middle sized shapes were used for training, the others being used to control for discrimination by length of contour, brightness or "subjective size" as previously described (Dodwell, 1957). The rats were trained in the jumping habit on a brightness discrimination. Methodologically, the fact that rats have to be trained to jump before they can be taught shape discrimination is of some importance, since in previous work with the jumping stand pre-training has always been given which could involve discrimination by shape or orientation. Thus, it is usual to train rats to jump initially to an open door, which could involve discrimination of the horizontal base of the rectangular aperture, and hence might predispose the animal to select a shape with horizontal base. In the present experiments the rats were given no training on jumping to open doors, and the brightness discrimination used was such as to preclude any cues of a "shape-like" nature. The rats learned to discriminate a diffuse pattern of white dots on a black card from a plain black card. The dots were not randomly scattered, but concentrated in the centre of the card to form an "aiming point"; from this point the

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gradient of brightness (average separation of dots) was approximately equal in all directions. It might be objected that to pre-train the rats on a brightness discrimination would prejudice them in favour of attempting to discriminate on the basis of brightness, or brightness distribution (Dodwell, 1957) in subsequent training; the first of these possibilities is ruled out by control of brightness in subsequent discriminations, the second cannot be *completely* eliminated, and its possible effects are discussed below. Using the jumping stand technique it is necessary to teach animals to jump by first allowing them to push down the doors, and this involves close contact with the stimulus shapes; in the present experiments shapes are to be presented in such a way that they only subtend a small visual angle at a rat's eyes. It is therefore necessary to have the jumping habit established before proceeding to the discrimination of shapes (from a distance).

Experiments

Experiment I. The purpose of this experiment was to investigate both the rat's capacity for discriminating between small shapes, and Lashley's finding that rats have difficulty in discriminating a square from a circle (a finding shown to be incorrect under certain conditions in the writer's experiment mentioned above). A "small shape" is used here to denote a shape which subtends a visual angle of about 10°, i.e. approximately $\tan^{-1}\frac{1}{6}$ for a shape with diameter 1 in. viewed at about 6 in.

There were ten subjects. On the basis of their performance in the initial brightness discrimination they were divided into two equal groups, group I being trained on the discrimination of triangle from circle, group II on square and circle.

It was found that the discrimination of small shapes under the given conditions requires a large number of trials, and that—within the number of trials given in the experiment—the discrimination is not learned to the criterion of twenty errorless jumps, a criterion previously employed in this type of work by Lashley and others (Lashley, 1938; Dodwell, 1957). Two of the rats (one from each group) had to be discarded because of their poor condition, and one other rat from group II was discarded because of the frequency of badly executed jumps. Training was terminated at the 400th trial for all animals, and their performance over the last fifty trials examined.

A simple χ^2 test on the last fifty jumps for each animal showed that two rats in group I had better than chance scores ($0{\cdot}01 < p < 0{\cdot}05$) and none in group II. It would seem then, *prima facie*, that there is some support for the statement that triangle-circle (group I) is easier than square-circle (group II) discrimination. During training it had seemed that some rats in group II were performing well, so the same test was applied to the previous fifty jumps (Nos. 301–350) for each animal. This showed that two rats in group II had performed at a level "better than chance" (in each case p of χ^2 was between 0·01 and 0·05) although none of the other animals had done so. These results are summarized in Table I, where the individual χ^2 have been

TABLE I

Group	Discrimination: Positive	Discrimination: Negative	No. of trials	χ^2 Trials 301-350	χ^2 Trials 351-400
I (4 rats) ..	Triangle	Circle	400	N.S.	$p < 0{\cdot}05$
II (3 rats) ..	Square	Circle	400	$p < 0{\cdot}05$	N.S.

added to give group results. The fact that the performance of rats in group II had deteriorated is of some interest: it may have been due to progressive deterioration in their condition as the experiment progressed (the feeding schedule was somewhat too

rigorous, and was changed in subsequent experiments): however, Wells and Wells (1956) and Sutherland (1957*a*) have also observed falling off of success in a difficult discrimination in the octopus. The level of performance of the animals which achieved better than chance scores on the last fifty trials was not sufficiently consistent to make it likely that the simple transfer tests used in earlier work (Lashley, 1938; Dodwell, 1957) would yield any useful information. It should, however, be possible to estimate transfer effects by giving additional *training* on new discriminations, and measuring relative speed of learning for different pairs of shapes (see experiment II below). In view of the poor condition of the rats, no such further training was attempted.

The results of experiment I may be summed up as follows:—

(1) The discrimination of small shapes is not readily attained by rats in the jumping stand, when boundary effects are reduced and when no pre-training on shape discrimination has been given. When attained, such discriminations may be unstable.

(2) There is no evidence that the rats used could discriminate more easily between triangle and circle than square and circle, although it may be assumed that Deutsch's system of shape recognition, which predicts confusion of square with circle, might be operative under the conditions of the present experiment (*vide* Introduction). The results are compatible with the writer's theory, which makes no firm prediction about the relative difficulty of the two discriminations; either theory might be held to predict difficulty of discrimination of small shapes if there is noise in the "final common cable" (Dodwell, 1957). The experiment supplies evidence incompatible with Deutsch's theory, but gives no specific support to the writer's theory. An attempt to confirm a prediction from the latter theory is made in the next experiment.

Experiment II. A prediction from the writer's theory previously made (Dodwell, 1957) is that a small equilateral triangle (base horizontal) should be confused with an inverted similar triangle, but should be readily discriminable from such a triangle which is positioned with one side vertical. The aim of the experiment is to test this prediction.

There were eight subjects in the experiment. A new schedule was introduced, in which the feeding time was increased to four hours per day; in all other respects the handling and pre-training of the rats were the same as in Experiment I.

All the rats were trained on the discrimination of triangle from circle, but only four showed evidence of discriminating after 400 jumps. These animals had required an average of 393 jumps (range 382–412) to reach a criterion of 18 out of 20 correct, and individual χ^2 over the last fifty jumps had $p < 0{\cdot}01$ for each animal. Thus there can be little doubt that they had fairly mastered the discrimination. The successful rats were divided into two groups on the basis of their performance over the last fifty training trials. Group I was set to discriminate an inverted triangle from a circle; group II to discriminate a triangle with one side vertical (or, what amounts to the same thing, the original triangle rotated through 30°) from a circle. (Simple transfer tests, with both doors unlocked, were not attempted, for reasons indicated above.) It will be recalled that the prediction is that group I will perform significantly better than group II. Each rat was given seventy trials on the transfer discrimination. No rat showed evidence of transferring straight away to either of the transfer triangles, but over the last thirty trials the performance of the two rats in group II was signi-

ficantly above chance expectancy on a χ^2 test ($p < 0{\cdot}05$). These results are summarized in Table II. The results of the transfer training therefore clearly refute the prediction from the writer's theory.

TABLE II

Group	*Discrimination*		*Number successful (average number of trials = 393)*	*Transfers*	χ^2
	Positive	*Negative*			
I (4 rats) ..	Triangle (base horizontal)	Circle	2	Inverted	N.S.
II (4 rats) ..	Triangle (base horizontal)	Circle	2	30°-rotated triangle	$p < 0{\cdot}05$

However, there are several possible explanations of this finding which could obviate the necessity of outright rejection of the theory: (*a*) It could be that in original training on triangles and circles the rats had learnt a "gradient of brightness" discrimination, despite the fact that outline figures were used, rather than "filled in" figures. (The possible confounding effects of using filled in figures in shape discrimination experiments are discussed in a previous paper (Dodwell, 1957).) This would leave unexplained the fact that group II learned the transfer discrimination, since rotation of the triangle through 30° reduces a brightness gradient in the vertical dimension to one very nearly the same as the gradient for the circle. Thus if the rats had been discriminating by means of brightness gradients one would have expected (if anything) less learning in group II than in group I. (*b*) It might be that rotation through 30° is insufficient to disrupt the original discrimination to any great extent. This seems unlikely if the writer's theory holds, and does not accord with other experimental findings on the effects of rotation of shapes (Fields, 1932). (*c*) It might be that the discrimination of the inverted triangle from a circle is for some reason inherently more difficult than the discrimination of a 30° rotated triangle from a circle, so that any facilitation from the learning of the original triangle-circle discrimination would be masked. (*d*) It might be that the shapes used, and the conditions of the experiment (i.e. small shapes against a large homogeneous background) were not sufficient to preclude the complicating factors mentioned in the introduction. If this is the case, it is unfortunate, since rats would certainly have great difficulty in discriminating still smaller shapes.

The third possibility (*c*) seemed to the writer the only one worth investigating further, which is done in experiment III. If this possibility turns out to be wrong, then a definite piece of counter-evidence will have been produced.

Experiment III. There were eight subjects in this experiment. The feeding schedule was the same as for experiment II. The rats were divided into two equal groups at the end of the initial training period, as before. Group I was trained on the discrimination of inverted triangle from circle. Group II was trained on the discrimination of 30°-rotated triangle from circle. The course of learning was similar to that found in experiment II. There were no obvious differences in the

learning rates of the two groups up to 400 trials, when training was terminated, nor did statistical analysis reveal a difference; one can therefore be reasonably confident that the discriminations are of about the same difficulty. This means that the better transfer to the 30°-rotated triangle discrimination in experiment II cannot be explained in terms of the relative difficulty of the two discriminations; hence it must be a transfer effect, which is opposite to the transfer predicted in the writer's theory.

Discussion

The results of experiments II and III cast doubt on the correctness of the writer's theory. No other theory of shape discrimination, so far as the writer is aware, would predict the differential transfer effect of experiment II; it certainly would not be predicted in Deutsch's modified theory (Deutsch, 1958) nor in Sutherland's theory (Sutherland, 1957*b*) even if this latter could be interpreted in terms of contour discrimination.

There is a good deal of evidence to support the contention that a system of shape discrimination similar to that postulated in the writer's theory is operative in the analysis of shapes in simple visual systems (Dodwell, 1957, 1958), which makes it reasonable to look for further evidence before rejecting the theory entirely. The writer has not been able to devise a suitable modification to his theory which would encompass the results here reported.

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CONTEXTUAL CUES IN SELECTIVE LISTENING

BY

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Two messages were presented dichotically and subjects were asked to "shadow" whatever they heard on one ear. Somewhere in the middle the two passages were switched to the opposite ears. Subjects occasionally repeated one or two words, at the break, from the wrong ear, but never transferred to it for longer than this. The higher the transition probabilities in the passage the more likely they were to do this. One explanation might be that the "selective filter" (Broadbent, 1958) acts by selectively raising thresholds for signals from the rejected sources rather than acting as an all-or-none barrier.

Introduction

Cherry (1953) found that subjects, when asked to "shadow"—or repeat aloud continuously as they heard it—a passage of prose given to one ear, remained almost completely unaware of the content, though not of the presence of another passage in the other ear; they noticed gross changes of pitch or loudness, but not the introduction of a foreign language or of reversed speech. He concluded that only certain statistical properties of the sounds were analysed, but none of the meaning of the words. Moray (1959) studied in more detail the nature of the attention "barrier." He found that subjects, given a recognition test, failed to show any trace of repeated lists of words given to the rejected ear, and that when they were given a specific set to listen, for example for numbers, they were no more likely to hear them than if they were given more general instructions. The only signals which were sometimes heard were the subjects' own names. He suggested that some kind of analysis was carried out prior to the level of the selective filter, but that only "important" or affective signals were allowed to pass.

It has been shown that dichotic localization of the sound sources is a very effective cue for selective attention (Broadbent, 1954); if both passages are given to both ears it becomes very much more difficult for subjects to separate out the two passages, at least on the first trial (Cherry, 1953). Another factor which plays an important part in "shadowing" tasks is redundancy in the message itself. Moray and Taylor (1958) asked subjects to repeat as they heard them passages composed of statistical approximations to English (made up using Miller's technique (1950)) and found that the number of words omitted was logarithmically related to the order of approximation to English. The higher the transition probabilities between the words, the more likely they were to be heard and correctly repeated.

The aim of the present experiment was to discover whether this type of contextual cue, or expectancy based on transition probabilities between words, would be strong enough to over-ride the dichotic localization cues; whether, if words were made highly probable instead of "important" (as in Moray's experiment), they would also be allowed through the selective attention filter, despite the fact that they came from the rejected ear.

Method

The apparatus used was a Brenell Mark 5 two-channel tape-recorder, the output from each track of the tape going through independent amplifiers to separate earpieces of a pair of headphones. Each subject was asked to equalize the intensities of the two passages.

The passages recorded were fifty words long and were of four different kinds: (*a*) narrative passages from a novel (*Lord Jim* by Conrad), (*b*) extracts from a technical discussion of language (from *Signs, Language and Behaviour* by C. Morris), (*c*) eighth order statistical approximations to English, (*d*) second order approximations to English. There were twelve passages of types (*a*), (*c*) and (*d*) and four of type (*b*). The passages were recorded in pairs, one on each track of the tape, in the following arrangements:—

a–b, b–a, a–c, c–a, a–d, d–a, c–d, d–c, c–c, d–d;

two examples of each pair were used, and the order was randomized. At some point in each recording between the twentieth and the thirty-fifth word, the passages were switched from one track to the other, so that each recording consisted of, on one track, the first part of passage 1 and the second part of passage 2, and, on the other track, the first part of passage 2 and the second part of passage 1. The switches from top to bottom and bottom to top track of the tape-recorder were made to coincide and any pause or change of tone was, as far as possible, avoided. The following example, an "*a–d*" pair, is typical:—

1*st track*. "While we were talking she would come and go with rapid glances at us
2*nd track*. "The camera shop and boyhood friend from fish and screamed loudly

leaving on her passage an impression of grace and / is idiotic idea of
singing men and then it was jumping in the tree / charm and a distinct

almost there is cabbage a horse which was not always be the set
suggestion of watchfulness. Her manner presented a curious combination

works every evening is heaviest with bovine eyes looking sideways. . . ."
of shyness and audacity. Every pretty smile was succeeded swiftly by a . . ."

The 18 subjects were undergraduates or research students. They were all given three practice passages of 100 words to shadow. They were then given the passages described above, one track to each ear, and were asked to repeat as they heard it whatever came through one of the two headphones: some were asked to listen to the right and some the left ear.

Six subjects were also given a control series (half of them before the experimental series and half after it). Here the top track was exactly the same as that described above, but on the bottom track a different and irrelevant passage of narrative was recorded, so that the subjects still heard the break in context, but the passage was not switched and continued on the other ear.

Their responses were recorded and later scored for any words repeated from the wrong ear. These were arranged in three categories: intrusions from the wrong ear made within five words before the break in context; intrusions within five words after the break and intrusions elsewhere in the passages. Subjects were also asked if they noticed anything about the rejected passages at the end of the experiment.

Results

There were relatively very few changes to the wrong passage: no subjects changed ears for the whole of the second part to follow contextual rather than localization cues. Three subjects never transferred to the wrong passage. However, fifteen of them did repeat just one or two words on one or more occasions from the rejected ear (the average number of words, for these fifteen subjects, out of twenty 50-word passages being about six). But, when asked afterwards, only one subject had any idea that the passages had been switched to opposite ears, and only two thought they might have said one or two words from the wrong ear. The one who realized the passages had changed sides suddenly commented towards the end of the series, "That time the right ear suddenly wanted to take over." All the other subjects described the rejected passage as "just noise," "perhaps English," or made similar comments. Several noticed that some at least of the passages they were repeating had a break in the context, which they found rather disturbing, and all realized that some (the statistical approximations) were more disconnected than others. Seven subjects were also given, after the other passages, two more where message 1 was in a

woman's voice and message 2 in a man's voice. All of these noticed that the voices switched to opposite ears, although they did not transfer with them in what they were repeating.

The six subjects who did the control series did not insert any of the words which followed after the break in the original passage, so that it is confirmed that in the actual experiment, subjects were genuinely transferring to the other channel and repeating words heard on the side to be rejected.

The number of words repeated from the wrong channel in the "switched" case did seem to bear some relation to the redundancy of the prose. In the results these intrusions are classified by the nature of the material to or from which the transfer was made.

The differences between the conditions were tested, using Student's "*t*" method, to see which were statistically significant.

Intrusions *from* passage on rejected ear.

After break.	Novel > 8th order.	Significantly different	$p = 0{\cdot}01$	$t = 3{\cdot}2$
	Novel > 2nd order	Significantly different	$p = 0{\cdot}01$	$t = 3{\cdot}6$
	8th order 2nd order	Not significantly different.		
Before break.	None significantly different.			
Not at break.	None significantly different.			

Intrusions *to* passage on accepted ear.

None significantly different, except Before break, 2nd order > 8th order, just significantly different, $p = 0{\cdot}05$, $t = 2{\cdot}6$.

Subjects were significantly more likely to repeat words from the rejected passage after the break if the context they were following was the narrative prose from a novel than if it was a statistical approximation to English, but no difference was seen between the 8th order passages and the less redundant 2nd order ones. There were some, though considerably fewer, intrusions before the break; this was possible because when subjects shadow they are repeating two or three words behind the recorded ones. When all intrusions in the five words preceding the break in context were summed and compared with all intrusions not at the break, divided by eight (to give equal numbers of words which could potentially have been transferred), the difference was statistically significant ($p = 0{\cdot}01$ level). Here, the contextual constraints of the passage which has just come to the right ear from the wrong one seem to work retrospectively, applying transition probabilities in the reverse direction. (Goldman-Eisler (1957) has shown that this is possible, using Shannon's (1949) guessing technique.) There were not enough of these intrusions to show any statistically significant differences between types of prose, although the numbers show the same trend as those coming after the break.

When the results were tabulated in terms of the numbers of intrusions *to* a certain type of passage, there were no significant differences between the different types of prose in intrusions after the break, or intrusions not at the break, perhaps because there were two factors acting in opposite directions: the greater the redundancy the less likely subjects were to leave the correct passage, but also the greater would be the disruption of transition probabilities when the break in context came. In number of intrusions before the break, there were just significantly more from 2nd order approximation to English than from 8th order, presumably an effect of its lower redundancy.

TABLE I

The table shows $\frac{\text{Number of intrusions}}{\text{Number of words} \times \text{Number of passages} \times \text{Number of subjects.}}$

1. Intrusions *from* passage on rejected ear.

	(*a*) *Novel*	(*b*) *Technical prose*	(*c*) *8th order*	(*d*) *2nd order*
After break ..	$\frac{27}{5\times6\times15}$ (6%)	$\frac{10}{5\times2\times15}$ (6·7%)	$\frac{9}{5\times6\times15}$ (2%)	$\frac{8}{5\times6\times15}$ (1·8%)
Before break ..	$\frac{8}{5\times6\times15}$ (1·8%)	$\frac{1}{5\times2\times15}$ (0·7%)	$\frac{2}{5\times6\times15}$ (0·4%)	$\frac{4}{5\times6\times15}$ (0·9%)
Not at break ..	$\frac{3}{40\times6\times15}$ (0·08%)	$\frac{2}{40\times2\times15}$ (0·17%)	$\frac{1}{40\times6\times15}$ (0·03%)	$\frac{8}{40\times6\times15}$ (0·22%)

2. Instrusions *to* passage on accepted ear

	(*a*) *Novel*	(*b*) *Technical prose*	(*c*) *8th order*	(*d*) *2nd order*
After break ..	$\frac{19}{5\times6\times15}$ (4·2%)	$\frac{9}{5\times2\times15}$ (6%)	$\frac{16}{5\times6\times15}$ (3·6%)	$\frac{10}{5\times6\times15}$ (2%)
Before break ..	$\frac{4}{5\times6\times15}$ (0·9%)	$\frac{2}{5\times2\times15}$ (0·4%)	$\frac{1}{5\times6\times15}$ (0·2%)	$\frac{8}{5\times6\times15}$ (1·8%)
Not at break ..	$\frac{4}{40\times6\times15}$ (0·11%)	$\frac{2}{40\times2\times15}$ (0·17%)	$\frac{2}{40\times6\times15}$ (0·06%)	$\frac{6}{40\times6\times15}$ (0·16%)

A few examples of the kinds of intrusion that occurred were:—

(1) ". . . I SAW THE GIRL / song was WISHING . . ."
. . . me that bird / JUMPING in the street . . .

(2) ". . . SITTING AT A MAHOGANY / three POSSIBILITIES . . ."
. . . let us look at these / TABLE with her head . . .

(3) ". . . THE GROWL OF THE / "GOAT" (go to) swim fast DURING THE . . ."
. . . book is she went to / thunder INCREASED STEADILY and the . .

(4) ". . . NEWER techniques will / FOR A MOMENT NOT DARING . . ."
. . . left, while I STOOD / be especially serviceable . . .

The words in capital letters were those spoken by the subjects.

Discussion

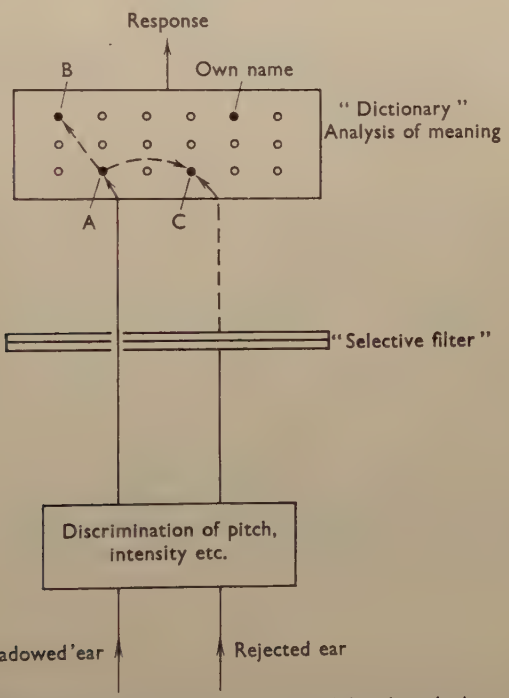
This experiment has confirmed the finding that when the two ears are used as the two channels in a selective listening task, subjects remain almost completely unaware of the content of the rejected passage. Moreover, it seems that contextual cues are not sufficient to make subjects change permanently to the second channel in order to follow the sense of the passage, or even to make them aware of what is being said there. However (using the terms and type of model put forward by Broadbent in his book, *Perception and Communication*) the "selective filter" does seem occasionally to allow one or two highly probable words through from the rejected channel, when the transition probabilities on the correct channel are suddenly contradicted.

The fact that subjects nearly all believe these words came from the same accepted channel makes it seem unlikely that the selective filter has been, as it were, reset momentarily for a different channel. Shadowing experiments suggest that there is a single channel system for analysing meaning, presumably comprising the matching of signals with some kind of "dictionary" and its store of statistical probabilities and transition probabilities gradually learnt through continual use of the language. If this is so, one should be able to avoid the "identification paradox" pointed out by Moray (1959). To explain his finding that subjects sometimes hear their own names when they occur in the rejected message, he suggests that there must be some kind of pattern analysis prior to the filter. Instead, one can suppose that in the "dictionary" or store of known words, some units or groups have permanently lower thresholds for activation, or are permanently more readily available than others: such might be "important" words, a person's own name, or perhaps danger signals (such as "look out" or "fire"); others would be lowered temporarily by incoming signals on some kind of conditional probability basis (along the lines suggested for a learning machine by Uttley (1955)). Thus, for instance, if the three words " I sang a" were heard, the stored trace of the word "song" in the dictionary would have its threshold considerably lowered. The thresholds might of course also be altered in the same way by other types of contextual constraint, by the selection of a verbal category, for example, as in Bruce's experiment (1956) on recognition of words masked by noise. Here the knowledge of the class of words (for example "types of food") allowed them to be recognized at a considerably lower signal-to-noise ratio.

In using the word "threshold" in this context, it is not necessarily meant to imply an intensity threshold, which might be one possibility, but simply that the unit is more or less likely to be activated by incoming signals, or that it is made more or less quickly available. Now if the selective mechanism in attention acts on all words not coming from one particular source by "attenuating" rather than

"blocking" them, that is, it transforms them in such a way that they become less likely to activate dictionary units, it might still allow the above classes of words, with their thresholds which were originally exceptionally low, to be heard. It is suggested that what happens in the experiment described here might be as follows: for the first word after the passages have been switched, two units will be activated in the dictionary, one by the signal from the "selected" ear and the other by the summated effect of the "attenuated" signal from the rejected ear and the lowered threshold due to transition probabilities following the previous word. Either of these units may be chosen for the response, or neither: (many subjects in fact omitted words after the break, perhaps because they had no cues to decide between the two active units). For the second word after the break, the situation is rather different. The transition probabilities will be lowering the thresholds of units following both the units which were last active, but the signals coming in will not be equally effective, since the filter is still operating to favour the selected channel. Thus at either the second or perhaps the third word, the subject will return to the correct ear, and the transition probabilities will be consistent with this until the end of the passage. (Fig. 1.)

FIGURE 1



The thresholds of words B and C are lowered by their high transition probability after word A. Word C is also activated by the "attenuated" signal from the rejected ear and is sometimes heard.

An alternative hypothesis is that the rejected messages are sampled or monitored occasionally. If the signals in the brief sample happened to coincide with a unit in the word-matching system which had been made more sensitive or more available by high transition probabilities, it might emerge in the final response. If not, the competing favoured signal from the selected ear would be the one repeated.

Either of these two possibilities seems a more economical system than any reduplication of analysis before and after the selective filter. Something along these lines also seems necessary to explain why not only a few "important" words, such as one's own name, may be heard from the rejected ear, but also any word which has been made contextually highly probable. If Moray wanted his suggested analysing mechanism prior to the selective barrier to cope with all these possibilities, it would need to be as complex as the one he places after it, at the level of conscious perception.

This might also provide an alternative way of explaining responses to classes of words, such as may occur in perceptual defence experiments. Broadbent (1958) suggests that the filter may be set to select classes of words; but it is difficult to see how a word can be recognized as belonging to a class on the basis of its meaning (the only characteristic common to the class) without already having been analysed individually. An alternative explanation would be that within the "dictionary decoding system" all words belonging to a certain class might have their thresholds raised or lowered relatively to the others. This differs from Broadbent's hypothesis simply in that the selective filter is here confined to acting on "physical" cues of intensity, time or frequency differences, while selection according to characteristics of meaning is done in advance within the analysing or "P" system.

The writer would like to thank Professor R. C. Oldfield who supervized the research, Dr. R. Davis for his helpful criticism and the Medical Research Council for financial assistance. She is also grateful to all the volunteer subjects for their help.

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APPARATUS

THE USE OF SMALL FILAMENT LAMPS AS VISUAL STIMULI

BY

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The purpose of this note is to draw attention to the fact that the time needed for small filament lamps to reach full brightness is dependent upon the time for which they have been previously switched off. This dependence is a possible source of error in biassed choice reaction sequences, and a circuit is given which avoids the difficulty.

INTRODUCTION

Small low voltage filament lamps are very convenient and cheap sources of visual signals, e.g. in work on reaction times. It is widely realized, however, that there is a delay between the time at which such a lamp is switched on, and its achieving full brightness. In consequence, alternative and perhaps less convenient signals are often used instead. For example, neon lamps have no such delay but are of comparatively low brightness and of a colour which may be unacceptable.

There are cases in which the type of display needed makes the use of small filament lamps almost essential. One such case is the recently introduced "in-line display" in which digits or other characters are projected on to a small screen. This makes use of 6 volt 2 watt lamps with bunched filaments of high surface brightness. In preliminary work on an experiment on choice reaction times, the delay in achieving full brightness with these small lamps has been measured and a circuit devised which ensures that this delay is less than 0·01 sec. in all conditions.

MEASUREMENTS

A lamp from an in-line display unit was mounted close to a photoelectric cell in a dark enclosure, and its output was recorded by photographing a cathode ray tube display onto continuously moving film together with a 100 c.p.s. time marker which was switched synchronously with the lamp. The scale of the final projected image was such that the least time interval which could be noted was 0·001 sec., and the least change in brightness 2 per cent. of the maximum value.

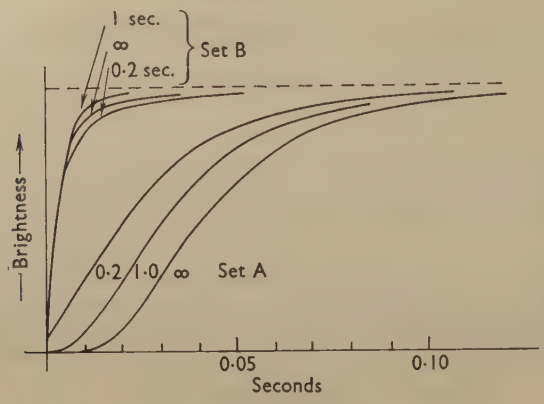
RESULTS

Figure 1 is a superimposition of tracings from the initial measurements, set A, showing the development of brightness with time when the lamp was supplied from a battery of low internal resistance, together with comparable tracings, curve set B, obtained when the circuit of Figure 2 was used. These traced curves are representative of very many obtained, all of which show very close agreement with one another.

The three curves of set A show, first, in the curve marked ∞, the normal switching on of the lamp, after it had been switched off for some ten minutes. It will be seen that the time delay involved is between 0·03 and 0·07 sec., the exact time depending upon how bright the lamp has to be before the subject sees it as "on," in the particular

circumstances of the experiment. This will be the condition in a choice reaction experiment in which the same signal is not repeated directly.

FIGURE 1



If, however, the experiment does involve repetition of the same signal, then the other curves of set A will be applicable. They show that when the lamp has been switched off for only 1·0 sec. or less, then the delay involved in restoring brightness after switching on again is reduced appreciably. When the lamp is switched off for an interval less than 0·2 sec., the corresponding curve does not start from zero. The filament is still hot enough to be emitting light when the circuit is once more made, and it is therefore to be expected that restoration of full brightness would be more rapid. The considerable difference which was found between the three conditions shown in Figure 1 set A was, however, rather unexpected and it shows that there was still appreciable heat in the lamp, even after an interval of 1·0 sec.

In the work which was being planned, it was hoped to use an in-line display in such a manner that individual lamps could be switched on either after an interruption of less than 0·1 sec., or after several seconds, or intermediate cases. Thus it was necessary to devise some means of reducing the resulting variability in the delay.

The circuit shown in Figure 2 was developed, and the curves shown in Figure 1 set B show the behaviour of the lamp in this circuit in the three conditions of set A. It will be seen that 80 per cent. of the full brightness is restored in less than 0·01 sec. in each of the three conditions shown .

The Circuit

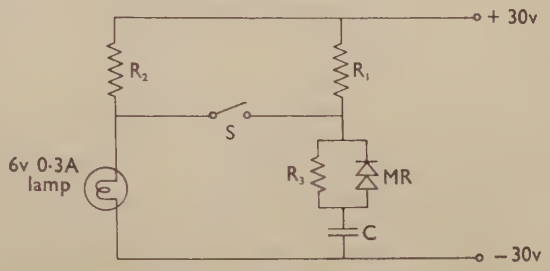
There are three parts to the circuit arrangement shown in Figure 2. Firstly, if R_2, R_3, and MR are excluded, C being joined directly to R_1, a simple condenser arrangement remains. By selection of C to suit a particular value of off period, this simple arrangement is sufficient to ensure a very rapid development of brightness, on switching on. If an experimental arrangement will not demand off periods of less than 1 sec., for example, the other components are unnecessary.

With this arrangement, however, there is still great variability resulting from variation in off period. Short off periods result in appreciable overshoot of brightness, while longer ones result in an initial step up to partial brightness, followed by a much slower approach to the full value.

The second stage in developing the circuit consists in the provision, by R_2, of a small standing bias for the lamp, when the switch is open. This bias of 0·06 amp

develops 0·5 volt across the lamp, and keeps the filament barely visibly red. With this arrangement, the initial rapid cooling of the filament is unchanged, but after about 0·6 sec. there is no further cooling, as is shown by the constant delay on switching on. This circuit would therefore be sufficient if the off period was always longer than 0·6 sec. Shorter off periods result in brightness overshoot.

FIGURE 2



The third development of the circuit is the introduction of MR and R_3, whose effect is to slow the recharge of the condenser C, when S is opened, so that this more nearly matches the decay of brightness of the lamp. It will be seen from Figure 1 that the curve corresponding to an off period of 0·2 sec. no longer shows overshoot, but instead is below the 1·0 sec. curve. These two curves could be made to coincide by the use of a lower value for R_3, but in this case, some overshoot occurs at about 0·5 sec., and the value given is considered to be that more generally useful. The function of MR is to ensure that on closing S and switching on the lamp, R_3 is effectively short circuited.

Conclusion

The circuit of Figure 2 is presented therefore, as an arrangement which will for practical purposes remove the disadvantages of small filament lamps, namely the slow onset of brightness on switching on, and the dependence of this upon the previous off period.

Component values

R_1 .. 100 ohms 7·5 watts.
R_2 .. 500 ohms 2 watts.
R_3 .. 470 ohms 1 watt.
C .. 150 microfarad, 50 volt.
MR .. 30 volt, 1·5 amp. half wave.

Dr. W. E. Hick used a simple condenser circuit in experimental work (Hick 1952) and the present circuit is merely a development of his idea.

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BOOK REVIEWS

The Annual Review of Psychology, Vol. 11. Edited by P. R. Farnsworth and Q. McNemar. Annual Reviews Inc., Palo Alto, California. 1960. Pp. ix + 544. $7.00.

I used to think that Sidney Smith was joking when he declared "I never read a book before reviewing it; it prejudices me too much." Now that I have finished the *Annual Review* I am not so sure. The principle behind this book is wholly admirable; no one could possibly read for himself more than a tithe of the psychological books and articles which are published every year, and an annual review giving a brief but lucid account of new developments and discoveries in the various branches of psychology would be of immense service to many people—to specialists wishing to keep in touch with departments of the subject other than their own, to students of related fields, and above all to teachers of psychology. But the book itself is very far from admirable; it is constipated with facts, and is rarely relieved by a flash of humour, a felicitous expression or an original thought. Perhaps its shortcomings are as much the fault of the editors as of the contributors. One suspects (rightly or wrongly) that the contributors were told to compress as much material as possible into the small space allowed them, and to keep their own opinions in the background. Comprehensiveness and impartiality are worthy goals, but they are beyond human reach. It is time that the editors bowed to reality, and admitted that the pursuit of forlorn ideals has cost the *Annual Review* a key position which it might well have occupied.

Articles which are too scrupulously impartial are prone to be dull, but dullness is a venial sin. On the other hand, articles which are overly comprehensive are painful to prepare, but leave their readers unenlightened; through trying to say too much, they often fail to say anything properly. Such faults are particularly deplorable in a publication like the *Annual Review* which can only be supposed to interest non-specialists. Specialists could hardly profit from reading articles which summarize two or three years' work in their fields in twenty or so pages. Non-specialists would like to read clear accounts of such recent developments in each branch of the subject as are of general interest. If there are no interesting developments, there is nothing worth reviewing, and if there *are* interesting developments then a review should concentrate on them and describe them fully. Yet the contributors to the *Annual Review*, feeling, doubtless, that they must cover as much ground as possible, have to travel so quickly that they cannot dwell on landmarks, and indeed can scarcely even pause to point out what the landmarks are. They can on the average devote only five or six lines to each publication which they survey; in so small a space it is hardly possible to summarize an article usefully, let alone to add a critical comment which might be helpful to a non-specialist. One article gives eighteen pages of references for twenty pages of text. Several articles contain paragraphs which are simply lists of references, and many contain paragraphs which might just as well *be* lists of references:—

"Optimal numeral form continues to be a subject of interest. Squires (144) was concerned with optimal numeral shape for viewing under red illumination conditions, while Soar (143) compared several numeral shapes under short, 40 msec.-exposure conditions. Each author reached conclusions regarding optimal form as examined under his conditions" (p. 77).

Such paragraphs are quite useless to a non-specialist, who would have neither the time nor the inclination to seek out the articles referred to. A specialist might find the references useful, but then why not simply print the references, suitably classified?

Several authors did not hesitate to introduce obscure technical terms whose meanings they did not have time to elucidate. The sense of this passage may be perfectly clear to an engineering psychologist, but it is not at all clear to me:—

"Chernikoff, Bowen and Birmingham (35) compared tracking performance with a zero-order (positional control) and a fourth-order aided control system when the frequency of the input signal was varied. The results indicated a decided superiority for tracking with the fourth-order aided system for low frequency input signals. However, as course frequency was increased, this superiority was lost, and at high frequencies of input the positional control system became superior. Although these results confound course frequency and system gain for the fourth-order aided control system, they stand as a striking illustration of how quickening can reduce an unmanageable control task to a level which permits highly accurate system performance" (p. 83).

There were only three articles which showed any real signs of breaking out of the general rut, namely, Hoffer's article on "Abnormalities of Behavior," Rotter's on "Psychotherapy," and Pribram's on "Theory in Physiological Psychology." Hoffer has made more effort than most to single out important developments, and his common-sense comments on some of the more pretentious articles reviewed are refreshing. Rotter manages to survey, without breaking into hopeless jargon, even the more esoteric kinds of psychotherapy, which is something of an achievement. Pribram's article is more ambitious than the rest; he attempts to relate recent discoveries in physiological psychology to recent advances in behaviour theory and in the construction of simulated brains. He covers a good deal of interesting material, and is not afraid to speculate. Unfortunately his exposition is in places exceedingly condensed and obscure and his account of simulated brains so short as to be virtually useless. One feels that either he should have been allowed the space to express his ideas more clearly, or else he should have restricted the scope of his article.

If this book accurately reflects the condition and prospects of psychology the outlook is decidedly dismal. It contains no hint of any major advances or revolutionary discoveries; and on the other hand it exemplifies a number of current tendencies which may become dangerous if not speedily checked. Especially worrying is the weight of jargon with which psychologists seem commonly to load their writings. The *Annual Review* contains not merely phrases, or patches, or even paragraphs of jargon, but whole pages of it. Jargon appears to have become the occupational disease of psychologists; its chief symptom is "a flux of words to the pen." The authors of the *Annual Review* are probably no worse stricken than most of the writers whose papers they summarize; but they illustrate a widespread and deplorable tendency towards slipshod writing. Why psychologists in particular should fall victims to the snares of verbiage I do not know; they have no excuse for slovenliness when they can turn for guidance to such masters of exposition as Watson and William James.

Of course every science must have its technical terms, but there is all the difference in the world between introducing a legitimate technical term, and substituting a long word for a perfectly useful short one. What is the point of coining words like "de-emphasizes," "self-devaluator" or "familial" in place of plain English phrases? In the *Annual Review* evidence from the replication of an experiment may be adduced as supportive of the theory which was originally hypothesized, and it may be hoped that as a result of further investigations a more comprehensive systematic integration of the evidence may eventuate. Such words impede the understanding without pleasing the senses.

I am afraid that I may be thought to exaggerate. I shall accordingly illustrate the leading characteristics of jargon by quotations from the *Annual Review*. In each case I have appended a rough translation in brackets; I make no claims that the translations are either perfect or unprejudiced.

1. *The Use of Circumlocutions, Clichés and Polysyllables.*

"The Editorial committee met at the Annual Reviews Building in Palo Alto to attempt long-range planning in the hope that a more adequate topical arrangement of the *Annual Review* chapters might eventuate." (The Editorial Committee met in Palo Alto to work out a new plan for the *Annual Review*.) This is the *very first sentence* of the Editorial Committee's Preface.

2. *Using the Passive Voice instead of the Active.*

"Progress in the scientific prediction of executive success is severely hampered by inability to solve the criterion problem . . . organization-rank and global-effectiveness ratings have served as success criteria in the field studies to date. Organizational rank is questionable on the grounds that non-merit selection considerations contribute unknown, but probably large, amounts of irrelevant variance" (p. 329). (Selection tests fail to predict the success or failure of executives accurately; they fail because most testers take an executive's rank in his organization as an index of his success, and executives who have done badly in tests may yet rise to high rank through having friends on the board.)

3. *Preferring Abstract Words to Concrete Ones.*

"McMurry posits a functional relationship between the structured, routinized character of banking operations and organizations and the personality configurations of most

bank employees" (p. 332). (Personality tests show that bank employees are like banks—careful and methodical.)

It is dangerously easy to wrap the leanness of one's thoughts in a cloak of solemn phrases, and then persuade oneself that such a voluminous garb must hide a substantial body. "Jones and Kohler (65) reported an experiment and its replication that examined the ease with which a subject learns arguments that favor or oppose his position on an issue. They found that the direction of an argument interacted both with the subject's position and the plausibility of the argument. Thus, prosegregation subjects learned plausible prostatements and implausible antistatements better than they learned implausible pro and plausible antistatements. Neutrals learned all types of statements equally well. Jones and Kohler conclude that individuals attempt to protect an attitude position by denying both implausible elements in their own beliefs and plausible elements in opposed beliefs. This conclusion seems plausible" (p. 500). ("We censure others but as they disagree from that humour which we fancy laudable in our selves, and commend others but for that wherein they seem to quadrate and consent with us." Sir Thomas Browne.) "McDavid concluded that these subjects were motivated to conform in order to avoid being different" (p. 486). [*Sic.*]

Not only does jargon solace writers whose thoughts are unremarkable; it is a splendid smokescreen for conceptual confusions. To illustrate: the word "information" has a precise meaning only within the framework of information theory, and outside that framework its meaning is far less exact. Yet these days even when it is used imprecisely it trails clouds of glory which it has captured on its scientific excursions; knowledge of results has become "feedback of correct information" (p. 496), and curiosity behaviour is talked of as a sort of hunger for information (p. 150). It is dangerously easy to pass from the precise to an imprecise use of the word without noticing the transition, and to talk of information as not merely conveyed by stimuli, but as "concentrated" in parts of them (p. 134)—as though bits of information adhered to a stimulus like berries to a bush.

I do not believe that the *Annual Review* faithfully mirrors the state of psychology; it compresses so much into so small a space that the picture it gives is a distorted caricature rather than a true representation. But, as I said at the beginning, a periodical review of progress in psychology *could* perform a valuable service; and the time will soon come when such a review is essential. I would like to offer three suggestions to the Editorial Committee of the *Annual Review*:—

1. They should abandon the ideal of a comprehensive review.
2. They should ask contributors to *select* a limited number of important topics, and to discuss them clearly, critically and at some length.
3. They should ruthlessly eliminate all jargon and all spurious technical terms.

If the *Annual Review* were reformed in these ways it would become a boon to all hard-pressed psychologists, and I should call down blessings upon it.

Alan Gauld.

Mechanisms of Colour Discrimination. Edited by Y. Galifret. Oxford. Pergamon Press. 1960. Pp. vii + 296. 63s.

This book, the verbatim transcript of an international symposium held in Paris in 1958, is well printed on good paper. Eight of its 296 pages are devoted to a well-ordered, critical and interesting account of the colour vision of insects by K. von Frisch, which is the best presentation of this subject known to me.

Having thus described its serious merits, I can treat the rest of the book with the levity that it deserves.

The participants in the symposium are divided into two classes called, in English, "Reporters" and "Discussants." Only the principal contributions of the former are listed by title in the table of contents, and there is no index. With the exception of an exposition by Hurvich of his opponent-pairs theory, the listed contributions of the Reporters are brief reviews of recent work in fields related, closely or remotely, to colour vision. None but von Frisch's is at the same time orderly, full, critical and in good literary style, and some have not one of these virtues. They are ephemeral lectures, good as lectures, but not designed to be published in a book.

Unmentioned in the table of contents are the minor contributions of the Reporters and those of the Discussants. These vary in length from ten or more pages down to a sentence or single word. Each is printed in the language in which it was uttered, about two-fifths being in English, two-fifths in French and one-fifth in German. Some of the macaronic dialogue provides plesant exercise for the mental agility of the reader. The

onger speeches of the Discusssants either describe unpublished original work, and are in effect hastily-written research papers unprovided with summaries, or are intended to remind the reader of work already published by the speaker. The shorter contributions are mostly genuine discussion, and it is from these that most pleasure and instruction will be gained—instruction not about science but about the scientists. The personalities of the speakers appear vividly in these short speeches. Lively enthusiasm and an unusual quickness of mind is to be seen in almost every paragraph of le Grand, and a subtle sense of humour in all that Galifret says; the characteristic talking-on-paper of Rushton has all three of these qualities. Every reader will be delighted by the exchanges between von Frisch and Medioni on the phototaxis of *Drosophila*, and between Galifret and Medioni on the merits of red, yellow and blue mice as a test for colour vision in cats.

G. S. Brindley.

Le Problème des Névroses Expérimentales. By Jacques Caïn. Bruges. Desclée de Brouwer. 1959. Pp. 168. 150 Fr.b.

The considerable body of knowledge about experimental neurosis accumulated during the last fifty years has been largely the work of independent investigators, who have made little attempt to collate their findings. Pavlov, Liddell, Maier, Masserman, Miller and many others have all studied phenomena which could be labelled as experimental neurosis, but each has used different animals and experimental techniques. In consequence the behaviour shown, the terms used to describe it and the concepts invoked to explain it, have been diverse. In this book Dr. Caïn, who has himself contributed very significantly to the field, draws together this diverse work: to make a synthesis would be impossible, but he presents a clear outline of the problems involved. This he achieves by a careful review of the techniques used (which range from discrimination to audiogenic methods), the types of behaviour shown, the therapeutic measures found useful and the explanatory concepts employed.

In presenting this review he attempts to abstract common features from the varied work he describes. Although he avoids meticulously the pitfalls presented by the over-generalizations of earlier authors, and the natural temptation to equate behaviour disturbance in animals and man, he does seem to be borne on by a faith that there is a something in common between audiogenic seizures and the consequences of a too-difficult discrimination problem—a faith which the reader may not share. His final conclusion that explanations are not necessarily mutually exclusive, even when they use concepts ranging from "neurophysiological conflict" to "conflicts of meaning," will be well taken, and may also serve to emphasize the breadth of approach necessary for further progress.

R. A. Hinde.

Einführung in die Pharmakopsychologie. By Herbert Lippert. (Vol. 4 of *Enzyklopädie der Psychologie in Einzeldarstellungen.*) Edited by R. Heiss. Bern. Hans Huber. 1959. Pp. 256. Fr.DM 32.

This book represents a useful, but rather unequal, survey of the field which is so very much in the foreground of scientific interest now among both psychiatrists and psychologists. It is written by a German psychologist of no definite school and represents a broad-minded introduction, but not more than that. In some places the author abstracts only the literature; in others there is an attempt at a more critical digestion of results. If one asks what kind of psychology the author represents, one finds that he adheres to a peculiar mixture of German theoretical psychology of the old type, with experimental and biological viewpoints. Hence his chapters on the experimental side are often inadequate, but his reports on clinical topics, such as addiction, are somewhat better. One can hardly blame him that he has little to say in a paragraph on "developmental pharmacopsychology," and the same holds true when he reports on "pharmaco-characterology." However, the list of references to the literature is comprehensive and carefully selected; it contains almost 1,700 items and gives a great deal of work done outside German-speaking countries. It contributes to the value of the book which is certainly to be admired as a courageous attempt to summarize the field from the point of view of the psychologist.

W. Mayer-Gross.

INDEX OF AUTHORS

BOYLE, D. G. - - - - - - - - - - - - - 171
BRINDLEY, G. S. - - - - - - - - - - - - 110
BROADBENT, D. E., with Ladefoged, P. - - - - - - - - 162
CARPENTER, A., with Leonard, J. A. - - - - - - - - - 249
CONRAD, R. - - - - - - - - - - - - - 45
CROSSMAN, E. R. F. W. - - - - - - - - - - - - 1
DEUTSCH, J. A. - - - - - - - - - - - - - 54
DITCHBURN, R. W., with Pritchard, R. M. - - - - - - - - 26
DODWELL, P. C. - - - - - - - - - - - - - 237
EAYRS, J. T., with Moulton, D. G. - - - - - - - - 90 & 99
ELITHORN, A., with Halliday, A. M. and Kerr, M. - - - - - - - 72
ETTLINGER, G. - - - - - - - - - - - - - 41
GEORGE, F. H., with McIntosh, S. B. - - - - - - - - - 141
GILLIES, S. M., with Macsweeney, D. A. and Zangwill, O. L. - - - - 113
GRAY, J. A., with Wedderburn, A. A. I. - - - - - - - - 180
HALLIDAY, A. M., with Elithorn, A. and Kerr, M. - - - - - - - 72
HERMELIN, B., with O'Connor, N. - - - - - - - - - 48
HOWARD, I. P., with Perry, A. - - - - - - - - - - 121
HOWARTH, E. - - - - - - - - - - - - - 117
HULICKA, I. M. - - - - - - - - - - - - - 185
JOYNSON, R. B., with Kirk, N. S. - - - - - - - - - 221
KERR, M., with Elithorn, A. and Halliday, A. M. - - - - - - - 72
KIRK, N. S., with Joynson, R. B. - - - - - - - - - 221
LADEFOGED, P., with Broadbent, D. E. - - - - - - - - 162
LEONARD, J. A., with Carpenter, A. - - - - - - - - - 249
MACSWEENEY, D. A., with Gillies, S. M. and Zangwill, O. L. - - - - 113
MCINTOSH, S. B., with George, F. H. - - - - - - - - - 141
MORAY, N. - - - - - - - - - - - - - 214
MOULTON, D. G. - - - - - - - - - - - - - 203
MOULTON, D. G., with Eayrs, J. T. - - - - - - - - 90 & 99
MOWBRAY, G. H. - - - - - - - - - - - - - 193
MYERS, R. E., with Schrier, A. M. and Sperry, R. W. - - - - - - 65
NEBYLITSYN, V. D., with Rozhdestvenskaya, V. I. and Teplov, B. M. - - 17
O'CONNOR, N., with Hermelin, B. - - - - - - - - - 48
OLDFIELD, R. C. - - - - - - - - - - - - - 33
PERRY, A., with Howard, I. P. - - - - - - - - - - 121
PRITCHARD, R. M., with Ditchburn, R. W. - - - - - - - - 26
ROZHDESTVENSKAYA, V. I., with Nebylitsyn, V. D. and Teplov, B. M. - - 17
SCHRIER, A. M., with Myers, R. E. and Sperry, R. W. - - - - - - 65
SPERRY, R. W., with Myers, R. E. and Schrier, A. M. - - - - - - 65
STANILAND, A. - - - - - - - - - - - - - 149
SYLVESTER, J. - - - - - - - - - - - - - 231
TEPLOV, B. M., with Nebylitsyn, V. D. and Rozhdestvenskaya, V. I. - - 17
TREISMAN, A. M. - - - - - - - - - - - - - 242
WASON, P. C. - - - - - - - - - - - - - 129
WEDDERBURN, A. A. I., with Gray, J. A. - - - - - - - - 180
WILKINSON, R. T. - - - - - - - - - - - - - 36
ZANGWILL, O. L., with Gillies, S. M. and Macsweeney, D. A. - - - - 113

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CONTENTS

G. H. MOWBRAY. Choice Reaction Times for Skilled Responses 193

D. G. MOULTON. Studies in Olfactory Acuity. III. Relative Detectability of *n*-Aliphatic Acetates by the Rat - - - - 203

NEVILLE MORAY. Broadbent's Filter Theory: Postulate H and the Problem of Switching Time - - - - - - - 214

R. B. JOYNSON and N. S. KIRK. An Experimental Synthesis of the Associationist and Gestalt Accounts of the Perception of Size. Part III - - - - - - - - - - - 221

Shorter Articles and Notes

JOHN SYLVESTER. Apparent Movement and the Brown-Voth Experiment - - - - - - - - - - - - 231

P. C. DODWELL. Discrimination of Small Shapes by the Rat 237

ANNE M. TREISMAN. Contextual Cues in Selective Listening 242

Apparatus

A. CARPENTER and J. A. LEONARD. The Use of Small Filament Lamps as Visual Stimuli - - - - - - - - 249

Book Reviews - - - - - - - - - - - - 252

Index of Authors - - - - - - - - - - 256

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